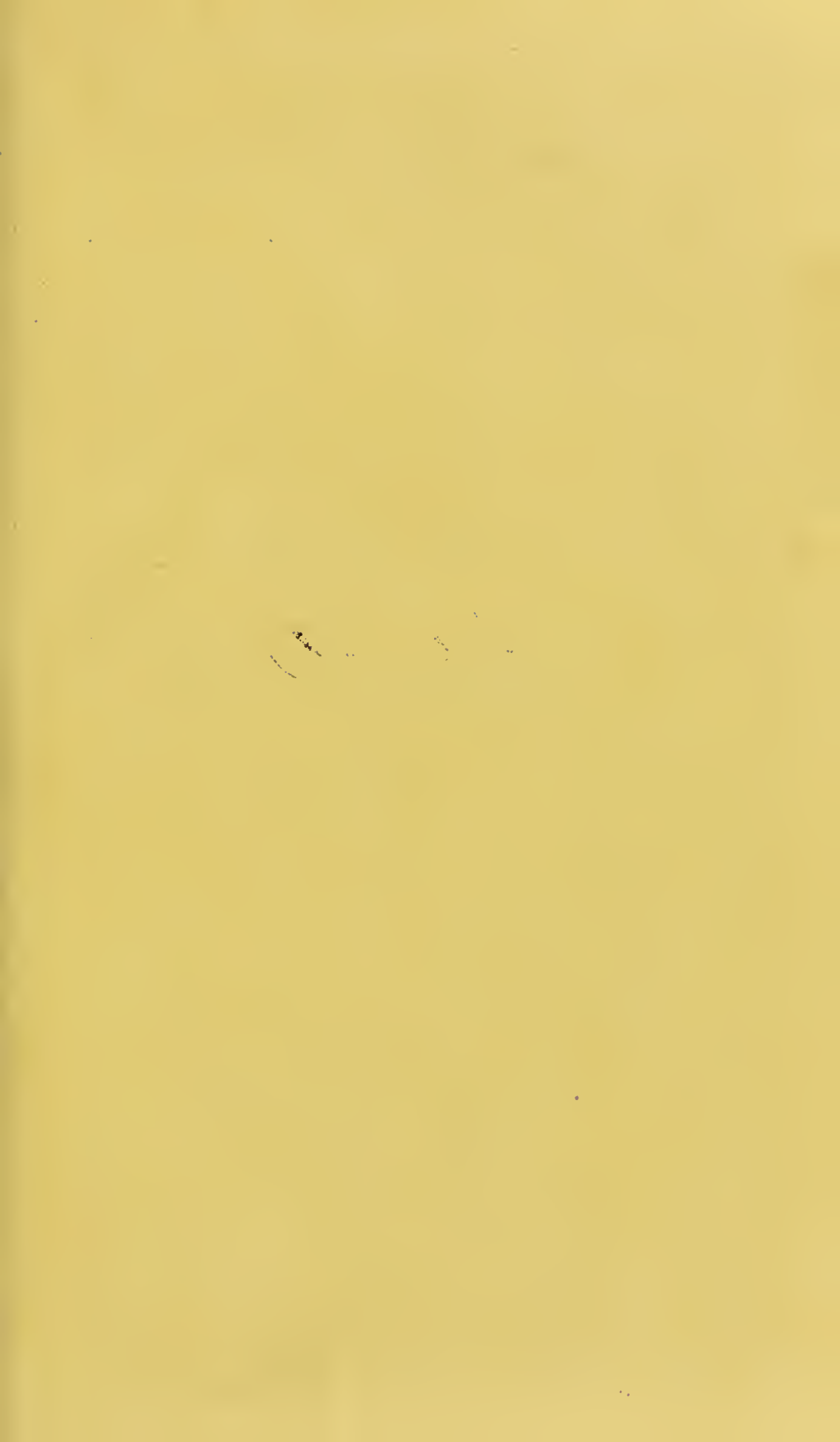


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THE PHILOSOPHY
OF THE
MOVING POWERS
OF
THE BLOOD.



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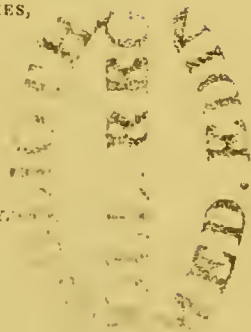
BY G. CALVERT HOLLAND, M.D.,

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"To discover truth is to do good on a grand scale."—BAILEY.

LONDON:
JOHN CHURCHILL, PRINCES STREET, SOHO.

1844.



J. H. GREAVES, PRINTER, SHEFFIELD.

P R E F A C E .

The attention of the author had been particularly directed to the phenomena of circulation as early as the year 1827, and the results of his investigations, at that time entered upon, were afterwards given to the public in a treatise entitled : AN EXPERIMENTAL INQUIRY INTO THE LAWS WHICH REGULATE THE PHENOMENA OF ORGANIC AND ANIMAL LIFE.* In this, original views were brought forward explanatory of changes in the distribution and qualities of the blood—views which met not only with a favourable reception from the leading medical journals in this country, but were alluded to in a gratifying manner by eminent writers both at home and abroad.

* Edinburgh : Printed for Macleachlan and Stewart, and Simpkin and Marshall, London, 1829.

In the study of the subjects which then fell under consideration, it appeared to him that the serious discrepancies in prevailing doctrines concerning the moving powers of the blood, were to be traced to the unphilosophical methods pursued in the investigation of them. They had not been surveyed in their wide and mutual relations to each other, hence the conditions induced in the experimental researches of the physiologist had a value and significance which were not understood. They were interpreted according to the prepossessions of the inquirer. By one they were imagined to demonstrate the transmission of the impulse of the heart to the remote columns of venous blood—by another the agency of the arteries, and by a third the inactivity of the capillaries. By others, however, the conditions were regarded as leading to very different conclusions.

An elaborate examination of the sanguiferous system tended to prove, that the diversity of opinion on these matters was clearly to be ascribed to the partial and illogical manner in which they had been contemplated. The understanding had looked upon the circulation rather in detached and independent parts, than as a system composed of several powers forming a continuous and comprehensive whole associated in the performance of the cardinal functions of life; in fact, so closely bound together that it is impossible to interfere with the action of any one without at the same time giving rise to an unnatural condition of the rest. It

would scarcely be supposed that a truth so obvious, and pregnant with such important considerations, would, in an age of stirring and restless inquiry, have been entirely lost sight of.

Entertaining these sentiments the author determined to devote the labour of years to a critical analysis of received doctrines, fully convinced that the undertaking, if carried out in an enlightened and impartial spirit, would lead to juster views—open a field—vast in extent and teeming with unexplored riches, the cultivation of which would throw new light on the origin, nature, and treatment of disease.

A knowledge of the varying conditions of the circulation, and the corresponding alterations in the qualities of the blood, is the only secure foundation on which theories can be built to direct practice. Every step which elucidates the laws governing these changes, is in the path of truth,—and is placing fresh powers in the hands of the scientific physician, by which he will see farther, and with a clearer vision, into the constitution of man.

When he had completed his inquiries, he submitted them to the judgment of friends well calculated to form a sound opinion of their value, and although this was favourable he hesitated in giving them to the world. Anxious to test, still further, their worth, they appeared in successive numbers of one of the first scientific medical

journals of the day,*—second to none in the elaborate character of its articles, and the ability with which it is conducted; and during the periodical publication of them, he received the most flattering commendations from some of the highest physiological authorities. They admit the originality and justness of his arguments. Thus strengthened in his own convictions he no longer delayed the appearance of his researches in the present form. They have, however, undergone great alterations—considerable additions have been made to them, and the whole has been most carefully revised.

During the progress of these inquiries the attention of the author was drawn to the study of the circulating system in the cardiac foetus, and the phenomena of arterial and venous action,—subjects, although not necessarily, nevertheless, intimately connected with the general scope of his undertaking, and on this he rests his apology for the introduction of them. The former has long been an interesting question in physiology—has exercised the ingenuity of minds of the highest order, and has given rise to numerous speculations. The difficulties which seemed to be inherent in it, were, perhaps, in his case, one of its attractions. The mind occasionally delights to grapple with what is intricate, obscure, or variously interpreted.

* Edinburgh Medical and Surgical Journal.

It has been remarked, and justly, by a philosopher, that “to discover truth, is in reality to do good on a grand scale. The detection of an error, the dissipation of a doubt, the extirpation of a prejudice, the establishment of a fact, the deduction of a new inference, the development of a latent principle, may diffuse its beneficial consequences over every region of the world. The great interests of the human race, then, demand that there should be no obstructions to inquiry, that every possible facility and encouragement should be afforded to efforts addressed to the detection of error and to the attainment of truth.”

To enforce truths, however, ahead of the seeming necessities of the age, is to receive either the contempt or the persecution of mankind. The former, if the prejudices, which are hugged with affection, are scarcely touched: the latter, if the truths strike at the root of prevailing opinions—expose the weakness and absurdities of doctrines cherished from their antiquity—the admiration with which they have been contemplated—the high names by which they are countenanced—the personal importance which they give, or the interests with which they are associated.

The inquiry which naturally follows the investigation into the moving powers of the blood, is the application of the principles developed to the discovery and treatment of disease. There is no serious deviation from health, but what is accompanied with corresponding modifications in the distribution and qualities of the vital fluid,

the knowledge of which will give a power—precision and foresight, at present unknown to the medical art. To furnish this information, in all its fulness of detail and copiousness of illustration, derived from experience, is a task of no ordinary character. The time, however, is fast maturing for the accomplishment of it. The necessity for more extended and accurate ideas on this subject is beginning to be felt—and to feel the necessity is to attain the object. The author will, perhaps, be allowed to say, that in all his published labours, he has ever steadily directed his talents to this end, and as a further illustration of its importance, he presents this treatise to the liberal consideration of his professional brethren.

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BOOK I.

INFLUENCE OF RESPIRATION ON THE MOTION OF THE BLOOD.

To whatever part of the animal economy the physiologist directs his attention, he meets with difficulties which are inherent in the nature of the researches, as well as arising from serious differences in the views of writers. The phenomena of life do not admit of exact calculation or of refined analysis, consequently the study of them, how successfully soever pursued, enables the understanding to seize only certain general facts, leaving in the field of investigation numerous important truths, which, in the present stage of physical science, elude the efforts of the inquirer. The circulation is no exception to the force of these remarks. If errors are to be inferred from the discrepancies in prevailing doctrines concerning it, they are certainly great, for scarcely on any two points are high authorities agreed. According to one, the motion of the blood depends on the heart alone; according to another, the influence of this organ is arrested at the capillaries: according to a third, there are three causes in operation—the heart—the

arteries, and the capillaries ; according to a fourth, these causes are insufficient to urge the blood through the venous system, and the effect is ascribed to the agency of inspiration. A fifth gives a moving power to the blood itself.

These differences of opinion have long imparted to the study of the circulation a peculiar interest. They are evidence of the obscurity in which the subject is involved, and on which it is important to possess just and enlarged views. A knowledge of the laws of the circulation is requisite in the judicious treatment of disease, and the more comprehensive it is, the more successful will be the practice employed. Without it how is it possible to economize, with safety and advantage, the expenditure of the vital energies, in the endeavour to correct striking deviations from health ? Or, in various constitutional disorders, to suggest and enforce the application of simple but efficient remedial measures ?

The great improvement which will ultimately be effected in the treatment of disease, will be derived mainly from the more extended views entertained concerning the laws of the circulation, in which is implied not only a knowledge of the moving powers of the blood, but of the various causes modifying the qualities of it. The importance of the inquiry, in relation to these objects, was early felt by me, and many years have elapsed since, in an especial manner, attention was directed to it.* Time and matured experience have strengthened the impression of its value. To remove the difficulties retarding the successful cultivation of this branch of science, to a large extent to be traced to the unphilosophical spirit in which it has been studied, is the aim of the present undertaking ;

* An Experimental Inquiry into the laws of Organic and Animal Life. Price, 12s. London: Longman, Rees, Orme, and Co. 1829.

and this will not be unprofitable, if it expose the errors of writers on a subject so practical, in all its diversified relations.

Were the circulation insusceptible of exact methods of investigation, such differences of opinion would be easy of solution. But physiology scarcely offers a department that admits more readily of experimental researches; and none in which they have been so varied or employed with equal ingenuity and tact. Nature, however, does not willingly yield up her secrets to such a rude mode of interrogation.

Experiments are often the source of much error, and give it a permanence and influence long fraught with injurious consequences—a truth which a recent writer has well expressed.

“Les expériences *ont, sans doute aussi, contribué beaucoup à l'avancement de la science*; mais combien de fois l'esprit s'est égaré à leur lumière vacillante et trompeuse! Rien de plus difficile que de bien interpréter le langage amphibologique des expérimentations: mille choses dont on suppose l'impuissance ou dont on ne remarque pas l'action, mille autres choses réellement impuissantes, auxquelles on accorde du pouvoir, y multiplient les illusions; portez sur elles l'œil de la critique, comme nous l'avons fait nous-même pour notre instruction, et votre conviction sera entière. Cependant il en est qui ont une utilité fort remarquable.”*

The object contemplated in this inquiry, is not only to analyze several of the cardinal functions of life, but especially the experiments by which they are generally imagined to be elucidated. The exposition of error is next in importance to the establishment of truth. It clears the ground for subsequent and more fortunate labourers.

* Essai de classification naturelle, &c., Par. P. N. Gerdy, p. vii.

The sources of error are indeed various. The physiologist, from whose writings the foregoing extract is taken, remarks further in reference to this subject :—

“ Cessons de reprocher entièrement aux physiologistes l'enfance de leur science, et de mettre les sciences physiques en parallèle. Les physiologistes ne pourront jamais arriver qu'à des résultats probables, tandis que les physiciens sont arrivés déjà à des certitudes. Cette différence est due à la nature du sujet dont chacun s'occupe ; et méconnaître cette vérité serait méconnaître l'une et l'autre science.

“ Voyons maintenant ce que peut le raisonnement. Appuyé sur les faits que lui a donnés l'observation, il en déduit une série de conséquences qui s'enchaînent les unes aux autres, et si l'esprit est assez sévère pour ne rien supposer, il ne se trompe jamais. Que les mathématiciens s'égarent en calculant la force du cœur ou d'autres muscles, c'est qu'ils supposent les premières données du calcul. Celui-ci pourrait être fort juste dans ses parties ; comme des suppositions en font la base, il ne serait pas étonnant qu'il fût faux, et que cependant il fut exact.

“ *Nos erreurs proviennent ainsi, toutes de nos suppositions.* Qu'on analyse tous les systèmes des hommes, on arrivera toujours à cette vérité. Nous supposons lorsque nous admettons un fait qui n'est pas, et nous supposons encore lorsque, par ignorance, nous rejetons un fait qui existe. Cependant si l'esprit s'égare à la lumière incertaine des suppositions, ses conséquences sont presque toujours dans un exact rapport avec elles, et ce sont alors de justes erreurs. Méfions-nous donc sans cesse de cette tendance de notre esprit à supposer.

“ Il ne faut pas non plus ne pas admettre *as is*, de peur de trop supposer. Ce scepticisme outré conduit nécessairement à des erreurs. Il ne faut pas se refuser à toute évidence qui ne frappe point les sens. Il est, par exemple,

des phénomènes secondaires qui ne proviennent jamais que d'un même effet primitif; ne peut-on pas alors déduire l'effet primitif de l'effet secondaire, à charge de se rétracter si cet effet secondaire pouvait provenir visiblement d'un autre effet primitif. Ainsi jusqu'à ce qu'on voie des fluides se mouvoir d'eux-mêmes, indépendamment de la pesanteur, des affinités chimiques et d'une force étrangère, ne peut-on pas assurer, lorsqu'on les voit s'agiter, qu'ils le doivent à une de ces puissances? Est-ce parce qu'on ne voit pas les capillaires qu'on peut les déclarer habituellement inactifs sur le sang qui y circule, et croit-on s'éloigner en cela de la marche des physiciens et des chimistes? Ce serait se tromper."*

The experimental physiologist too often flatters himself that his mode of investigation is open to few objections. Were his subjects susceptible of subtle analysis, like the materials of the chemist, or the inquiries of the mathematician, his self-complacency would be well-founded. The phenomena of life are, however, exceedingly complex, and so indissolubly associated in vital operations, that it is difficult to select a portion for experimental researches, without giving rise to serious and general modifications in them, the nature and extent of which are seldom accurately understood or even suspected.

The results obtained are viewed too much apart from co-existing actions, and are reasoned upon with as much confidence as if evolved out of inorganic substances. How seldom does the physiologist take into account the disturbing causes flowing from his interference! These are elements, indeed, which he does not allow to perplex or affect his attempts at generalization.

In the course of this undertaking, it will be shown that in numerous experiments, the amount of derangement, either

* Opus cit: p. xi.

general or local, necessarily produced in the vital powers, is a matter in no degree studied. The phenomena forced into notice are seized with avidity, and spun into theories with unhesitating boldness. If, in pointing out these defects and the consequences to which they lead, success does not invariably attend my labours, the instances of failure will not be altogether unprofitable. They will tend, from the spirit with which the investigation is conducted, to rouse and stimulate the mind to inquiry—to enlarge the field of its contemplations—to give additional interest to subjects of importance, and perhaps to impart a stirring feeling of independence to the cultivation of physiology generally. The man who dares to think for himself, unbiassed by great names, has overcome the most formidable difficulties opposed to the discovery of truth.

The circulation has high claims upon the consideration of every branch of the profession. It has an intimate relation to every vital operation, and the knowledge of its modifications suggests, in many diseases, prompt and appropriate measures; and, were it more accurate the remedial art would lose much of its obscurity, and humanity gain largely in beneficial results. The aim, scope and order of this inquiry are shown by the following heads:—

Book I. Influence of respiration on the motion of blood.

II. The properties and influence of arteries on the motion of blood.

III. The influence of the heart on the motion of blood.

IV. The forces by which the blood is moved in capillary vessels.

V. The powers by which the blood is moved in the veins.

Though the influence of respiration has been studied with elaborate attention, there are points of absorbing interest which have only been partially investigated, or which, indeed, are involved in much obscurity. Experi-

ments have unquestionably proved, especially those of Magendie, Poiscuille, and of Barry, that the motion of blood is greatly modified by respiration. They differ, however, in their views concerning the influence of the two acts, inspiration and expiration. The two former teach that inspiration only facilitates venous circulation. Barry regards it the sole cause. Previously to entering upon this part of the inquiry, the capacity of the arteries and veins, immediately connected with the heart and lungs, will properly form the first consideration. The greater capacity of the latter, as well as of the venous system generally, is familiar to all, but the fact has not been sufficiently appreciated, demonstrating, which it does, the provision made for the constant and equable supply of blood to the heart and lungs.

By injecting an equal portion of the aorta and of the two *venæ cavæ*, near the heart, a general idea of their relative capacity may be formed. The experiment will show that the capacity of the two veins is at least twice as great as that of the aorta; and as they allow, in various conditions of the animal system, of considerable enlargement, the difference will occasionally far exceed this statement. The area of the venous system is greater than that of the arterial, in the proportion of about three to one. The ratio between the capacity of individual arteries and veins in different parts is very various; between the carotid and internal jugular, 196 : 441; between the subclavian artery and vein, 3844 : 7396; between the aorta and *venæ cavæ*, 9 : 16; between the splenic artery and vein, 136 : 676. Nature, by the greater capacity of the veins, has secured the powers of life from frequent and serious accidents. Previous to illustrating this fact, compare the capacity of the *venæ cavæ* with that of the pulmonary artery. The difference is great, and yet as much blood passes through the artery at each contraction of the right ventricle, as flows

into the right auricle from the capacious veins, or into the aorta at each contraction of the left ventricle. The blood in these veins is moving slowly, for were it to circulate at anything near the same rate as the contents of the pulmonary artery or aorta, the right auricle would receive as much more blood, as the capacity of the veins is greater than that of either artery. No argument is required to show, that the same quantity of blood, in equal times, flows into the right side of the heart as passes out of the left. From these facts, it may be inferred,—

1.—That the motion of the blood is exceedingly slow in the *venæ cavæ*.

2.—That the quantity of blood immediately at their roots is much greater than is removed by any single contraction of the right auricle, and hence may be regarded as a reservoir at the right side of the heart.

The great difference in the capacity of the pulmonary artery, aorta, and *venæ cavæ*, must constantly be kept in view throughout this investigation; and also the fact, that the same quantity of blood passes in equal times through the two former, and from the latter into the right auricle. These are data from which important conclusions may be drawn.

The structure and functions of the lungs show that they are designed to receive an immense quantity of blood. Were it possible to inject the pulmonary vessels to ascertain the amount, the experiment would not lead to any satisfactory results, as it would not determine the quantity ordinarily in circulation. The division of the lungs into an infinite number of cells for the purposes of aeration, renders it probable that they contain many times the quantity sent out by any single contraction of the left ventricle, so that these organs may be considered as a reservoir to the left side of the heart, or, in other words, the left

ventricle would for some time be supplied with blood, were the contractions of the right to be arrested. This is not only a wise provision, tending to preserve existence under circumstances that would otherwise be fatal, but explains many interesting phenomena.

It has just been remarked, that, in equal times, as much blood is sent out by the left side of the heart as is received by the right. This is true only in health, or when the functions of the lungs are regularly performed. A disproportion in the amount transmitted and received, must necessarily take place in all cases of recovery from congestion of these organs. The idea of this disproportion occurred from the consideration of the following circumstance:—Having observed the great difference in the capacity of the *venæ cavæ*, pulmonary artery and aorta, it was natural to compare the pulmonary artery with the pulmonary veins. Knowing that these veins receive, in the same time, only as much blood as the artery transmits, though their conjoint capacity is greater, the question was suggested, what end is this greater capacity designed to serve?

The superiority of the venous system generally is obvious and admitted. The pulmonary veins are alluded to by physiologists, as an exception to the prevailing preponderance. Their united capacity is regarded as only slightly, if at all, superior to that of the pulmonary artery. There is certainly some difficulty in making a comparison, in consequence of the extreme shortness of the veins. An accurate investigation will not, however, establish such relation. Were direct evidence less decisive on this point, reasoning alone on the conditions of the blood, in these two classes of vessels, would render the opinion exceedingly questionable. The character of the stream emitted by the right ventricle differs very widely from that flowing through

the pulmonary veins into the left auricle. The former is urged along by the strong contractions of the ventricle, in quantity, and at a rate of motion, nearly corresponding with the current ejected into the aorta. The blood which passes into the left auricle is derived from capillaries, consequently its motion is slow and equable; and yet the quantity conveyed in a given time is identical with that transmitted by the right ventricle into the pulmonary artery. Hence the two streams—the venous which enters, and the arterial flowing towards the left auricle, are urged in their respective directions at a very different rate of motion. It will scarcely be imagined, that the venous circulates at the same velocity as the arterial current. The greater capacity of the veins throughout the animal system is admitted, nor is there an exception to the law. The difference is one of necessity.

The blood which enters the aorta passes gradually into branches, the conjoint capacity of which increases as they multiply, until at length they terminate in capillaries, the aggregate capacity of which is immeasurably greater than that of the aorta. It is therefore evident that equal quantities of blood are moving in these two situations at very different degrees of velocity. The venous capillaries pour their contents into vessels, the united capacity of which diminishes as they decrease in number, until they form the two ample *venæ cavæ*. An equal quantity of blood is consequently circulating at a greater rate of motion in the latter vessels than in the venous capillaries; but how slowly compared with its course in the aorta!

A difference in the capacity of the veins and arteries would naturally be inferred from the striking contrast in the relation of these vessels to the heart. The contents of the one are manifestly urged forward, by its contractions, with great force and velocity. The contents of the other

are far removed from this source of motion, and are separated from it by numberless capillaries, possessing a peculiar vital action, and unquestionably exerting an influence on the currents transmitted to them. According to this view, it would be expected that venous circulation would be much slower than arterial. This conclusion might indeed be arrived at from other considerations.

Nature displays wisdom in the construction of all her works. Design is stamped upon all her creations. The greater capacity of the veins is essential to the preservation of life in many trying situations. During fainting, the blood gradually leaves the arteries and accumulates in the veins : and whenever the vital energies are depressed the same effect takes place. This important modification in the distribution of the blood is imperatively necessary to the re-establishment of the powers of life. The left side of the heart and the arteries could not otherwise either begin to act or continue to carry on their feeble operations. In the case of fainting, the small quantities of blood sent into the aorta, on the reviving contractions of the heart, could not possibly urge forward the mass in advance of them and at rest ; and in simple depression of the vital powers the difficulties would only be less in degree.

The blood, during the enfeebled or arrested action of the heart, does not escape from one class of arteries only, but equally from arteries of the same calibre throughout the body. Situation does not materially influence the result. Whenever the heart ceases to contract, or its action is greatly weakened, the blood gradually leaves the arteries and accumulates in the veins. This effect takes place throughout the whole of the arterial system, in consequence of the interposition of capillaries between both classes of vessels, by which the blood is withdrawn from the one and transmitted to the other. Had not the veins a greater

capacity than the arteries, the contents of the latter would clearly be retained. The pulmonary artery may perhaps be adduced as an exception to the operation of this law, being often found distended after death. This does not, however, arise from the want of capacity in the corresponding veins to receive its contents, but from the influence of peculiar circumstances.

When the heart gradually ceases to contract, the arterial fluid in circulation becomes an exceedingly attenuated current, the veins and capillaries having already received the principal part. The surviving action of the latter vessels is, in this case, amply sufficient to absorb and convey forward the small quantities remaining after the cessation of the left ventricle. This cannot, however, be expected to occur in the lungs. Blood cannot circulate freely from the right ventricle to the left auricle when the lungs cease to be inflated, so that a portion sent out by the last feeble contractions of this ventricle will necessarily accumulate in the pulmonary artery and its larger divisions. The capillaries cannot possibly remove it. Their surviving action, in the vicinity of the pulmonary veins, frequently leads to distension of the left auricle. I am aware that in maintaining several of these opinions, I stand almost alone. There is one authority, however, on my side, the justly acknowledged weight of which makes me comparatively easy in my singularity. Bichat enforces the same doctrines. The following are his remarks on this subject:—

“Quant à l'arbre qui termine le système à sang noir, comparé à celui qui commence le système à sang rouge, ce n'est pas tout-à-fait la même chose. L'artère pulmonaire et les veines de même nom présentent une disproportion de capacité moindre, il est vrai, que dans les autres parties, mais qui est réelle, et qui, quoi qu'en aient dit plusieurs auteurs, est à l'avantage des dernières. Comment cela se

fait-il ? il semble que puisque l'une fait suite aux veines, qu'elle pousse le même fluide, elle devrait avoir la même proportion de diamètre ; et que puisque les autres se continuent avec les artères, elles devraient également leur être proportionnées. Cela dépend de la différence de vitesse du sang : en effet, ce fluide circule plus vite dans l'artère pulmonaire que dans les veines de même nom, puisqu'il y a l'impulsion du cœur dont ces dernières manquent : donc, dans un temps donné, il y passe en aussi grande abondance, quoique le diamètre de cette artère soit plus petit ; que dis-je ? s'il était égal, la circulation ne pourrait se faire. De même si l'aorte égalait en capacité les deux veines caves et les coronaires réunies, et que le sang y conservât la même vitesse, la circulation ne pourrait avoir lieu.

“Les veines pulmonaires sont un peu plus larges, étant réunies toutes quatre, que l'artère aorte, qui cependant transmet tout le sang qu'elle leur envoie. Pourquoi ? parce que l'impulsion que communique le ventricule gauche fait que, dans un temps donné, il passe plus de sang par l'aorte que par les quatre veins pulmonaires. Ces deux choses, 1°. vitesse du fluide, 2°. capacité des cavités où il circule, sont donc en sens inverse dans les deux arbres opposés qui forment chaque système vasculaire. Dans celui à sang rouge, il y a vitesse moindre et capacité plus grande du système capillaire pulmonaire à l'agent d'impulsion ; de celui-ci au système capillaire général, il y a au contraire vitesse plus grande et moindre capacité. Dans le système vasculaire à sang noir, il y a moins de vitesse et plus de capacité du système capillaire général à l'agent d'impulsion ; de celui-ci au système capillaire pulmonaire, il y a plus de vitesse et moins de capacité. Sans cette double disposition opposée, il est évident que la circulation ne pourrait avoir lieu.”*

* Anatomie Générale, Tome i. p. 351.

Magendie, in many parts of his writings, as in the following passage, asserts unequivocally the dependence of pulmonary circulation on the contractions of the right ventricle. Indeed, he ridicules the supposed co-operation of the capillaries in no measured terms :—

“ Nous vous avons indiqué les modifications qu’ éprouve le liquide animal dans ses propriétés physiques à l’instant où il traverse le poumon ; quelle est la force qui le met en mouvement dans ses tuyaux ? C’est la pompe pulmonaire. Chaque fois qu’elle se contracte, une nouvelle ondée de sang est lancée dans le système artériel et, par continuité de canaux, dans le système veineux. A cette cause d’impulsion il faut joindre la pression exercée par la colonne d’air et par les puissances expiratrices sur les organes contenus dans la cavité pectorale. Quant à l’action propre des capillaires, le resserrement actif de leurs parois, ce sont de ces rêveries auxquelles il ne faut attacher aucune valeur sous peine de nous ramener à l’âge d’or des propriétés vitales.”*

The investigations of this physiologist, every step of which he regards as sure, have led to errors not less serious than those which he attributes to refined and elaborate reasoning. He has appealed in his inquiries immediately to nature, but with too little consideration and caution. His method has, in most instances, been much too direct and coarse for the complicated powers on which he has operated. His experiments, to prove the transmission of the impulse of the right ventricle to the contents of the left auricle, will illustrate the justness of these strictures. They are thus minutely described by him :—

“ Les plumes qui recouvraient la face antérieure de la poitrine ont été enlevées : de cette manière on distingue

* Leçons sur les Phénomènes, &c., Tome ii. p. 266.

aisément les limites des os et des masses charneus. De chaque côté du sternum, je fais avec le scalpel une incision parallèle à la ligne médiane, puis avec le manche de l'instrument je gratte les surfaces osseuses pour en séparer les faisceaux musculaires. Il faut se servir le moins possible du tranchant de la lame de peur de blesser quelque vaisseau important. Chez les oiseaux, les hémorrhagies sont faciles, mais s'arrêtent presque toujours spontanément. Le sternum est dénudé, il s'agit maintenant d'en faire l'extraction. Ordinairement je désarticule cet os d'avec les côtes qui s'y attachent, mais ici, pour aller plus vite, je vais maintenant couper avec de fortes cisailles un large segment de la paroi pectorale. Vous entendez le cœur venir battre contre le sternum : quand je soulève la pièce osseuse, les battements cessent, et l'organe s'agite dans son enveloppe fibreuse par un mouvement de balancier. C'est par le choc alternatif de sa pointe et de sa face antérieure contre le thorax que j'explique le double son cardiaque : mais ce n'est point ici le moment de discuter cette question. Nous voulons examiner seulement comment le sang se meut dans les veines pulmonaires : pour cela, j'enlève complètement le sternum. La position profonde de ces vaisseaux, leur peu de longueur rendent ces recherches fort délicates : il faut soulever le cœur et le maintenir un certain temps dans cette position pour pouvoir isoler les veines. Je crains bien que nous soyons forcés de suspendre l'expérience. L'animal s'agite, se débat violemment : son cœur ne se contracte plus régulièrement, il n'offre que des palpitations péristastiques, signe certain d'une mort immédiate : en effet l'animal a cessé de vivre."*

This experiment is repeated, and with the same result. The animal in both cases dies, which is not strange,

* Tome supra cit : p. 272.

considering how cruelly it is cut up and tortured. The experiment is again repeated on a rabbit, respiration being maintained by artificial means, but with little better success. On puncturing one of the pulmonary veins, the jet of blood, in place of being synchronous with the contraction of the right ventricle, which was anticipated, is indeed synchronous with the contraction of the left auricle.

After these several failures, he discovers that the experiment is performed too near the left auricle to enable the observer to estimate the action of the right ventricle. Such researches clearly cannot afford any indication of the influence of the ventricle on the circulation of blood towards the left side of the heart, and yet the opinion is enforced with as much confidence as if founded on indisputable facts. Had the experiments succeeded, according to calculation, the results would have been comparatively of little value.

When the delicate and complicated functions of important organs are disturbed, as in this instance, it is impossible, amidst the confused phenomena presented, to distinguish between the natural and extraordinary. The direction of the circulating currents may be entirely changed. The contractile properties of vessels or their modifying powers, may for the time be destroyed, consequently the conditions induced, may differ widely from those in the undisturbed state of the vital functions. The truth of this is indeed borne out by the last experiment. The jet from the punctured vein was synchronous with the contraction of the left auricle—a result not in any degree anticipated. This shows how greatly the normal states of the pulmonary circulation were deranged. The reflux of the blood was occasioned by the difficulty opposed to its passage through the left side of the heart, and this cannot occur without distention in the series of vessels connecting the right

ventricle and the left auricle. If the fluid cannot escape from one end of the column it will necessarily accumulate in all the intermediate points, so that the pulmonary vessels will be in a condition unfavourable for the exercise of their contractile properties. The regurgitation from the contraction of the left auricle proves the existence of obstacles to the flow of blood in this direction, and establishes the distension of the series of vessels extending to the pulmonary artery.

Magendie in another of his works remarks, "That the contraction of the right ventricle is the cause that constantly keeps the elasticity of the sides of the artery in play; that is, which maintains them in a state of distention to such a degree, that, by virtue of their elasticity, they continually tend to contract and expel the blood."* In his experiments this necessary degree of distension is greatly exceeded, consequently, were the blood in the pulmonary veins observed to obey the impulse of the right ventricle, this would be no proof, as will be shown in a subsequent part of this inquiry, that their contents are directly influenced by it in the ordinary conditions of the circulation. In this, and in numerous other experiments, he reasons upon the phenomena evolved, as if they were necessarily the same in all states of the vital powers; and to this unphilosophical method of interrogating nature, may be traced the false conclusions at which he has arrived.

By the superior capacity of the pulmonary veins, nature has secured to the lungs every possible facility for the transmission of blood. It may perhaps be urged, that, as the cavities of the left side of the heart are not proportionately larger than those of the right, the greater capacity

* An Elementary Compendium of Physiology. Fourth edition, page 372, Translated by E. Milligan, M.D.

of the four veins cannot act in the manner supposed. The cavities of the heart do not, however, even in health, emit regularly the same quantity of blood at each contraction. This is modified by various circumstances, such as mental emotions—exercise—stimulants—sedatives, and almost by every kind of *ingesta*; hence the cavities of the left side of the heart, with this power of accommodation, may be regarded as possessing a capacity proportionate to that of the pulmonary veins, and when the necessity exists capable of propelling, in a series of contractions, a quantity of blood far greater than is sent into the lungs by a corresponding series of the right.

The left ventricle is much more liable to a thickening of its parietes, accompanied with dilatation, than the right, arising from its frequently accelerated and vigorous contractions. In a paroxysm of spasmodic asthma, the lungs are loaded with blood and mucous secretions. Alleviation of the distressing symptoms can take place only on a diminution of the congestion, but which could not possibly occur if the right side of the heart continued to furnish to the lungs as much blood as the left removed; for such equality would leave undiminished the accumulation. This reasoning applies not only to spasmodic asthma, but to all cases of pulmonary congestion. The right side of the heart has great difficulty at this time in circulating the blood, as is evident from the prominence and pulsation of the jugular veins. The many interruptions to the flow of it to and from the lungs, would frequently cause death, were not the *venæ cavæ* and the lungs reservoirs—the former increasing their capacity to receive and retain, until the heart is able to transmit forward a portion—and the latter supplying the left cavities until the balance of circulation is established. Hence the quantity transmitted by the right side of the heart to the lungs, is liable to be con-

tinually modified by the greater or less ease with which it permeates these organs.

The capacity of the capillaries of the lungs united exceeding many times that of the pulmonary artery, the blood has every facility for flowing from the artery into the lungs. It is also aided by another circumstance—the expansion of these organs in inspiration; but how far this promotes, or is necessary to the effect, is a point much disputed by physiologists. Barry contends, that the blood in the veins circulates only during inspiration. The contractions of the heart being about three times as frequent as the inspirations, the blood flows into the lungs during expiration, and consequently circulates in the veins. This fact does not, however, prove that the single quantities propelled by the successive contractions of the right ventricle, pass freely to the left auricle during expiration. The lungs contain a much greater quantity of blood than what is transmitted to them by several contractions of the right ventricle. There are no data by which the amount can be ascertained. Were it known, some general idea might be formed of the rate at which blood circulates in the lungs. According to the experiment of Hales, the velocity of the circulation in the lungs of a frog is five times greater than in the muscles; but this affords no information respecting the difference in the rate of circulation in human lungs and other parts of the body. The organs of circulation and respiration are simple in the one animal, and extremely complex in the other.

Were the capacity of the pulmonary capillaries known, or the quantity of blood which they contain, the rapidity of the circulation would be a problem comparatively easy of solution. It is on data of this kind that the rate at which blood moves in the aorta has been estimated, and probably

with considerable accuracy. Such evidence is, however, altogether wanting in this case, nor does it appear possible to obtain it.

The capillaries of the lungs have undoubtedly a conjoint capacity many times greater than that of the pulmonary artery, and the rapidity with which blood circulates in the arterial and venous systems being in the inverse ratio of their capacity, the difference in the motion of it in the pulmonary artery and in these capillaries will manifestly be great; and according to the same reasoning, the circulation in the pulmonary veins will be slow and equable, compared with it in the artery. The flow of blood from the right ventricle to the lungs is intermittent. The artery is too short to exert much influence upon it during the diastole of this cavity. The circulation from the capillaries to the left auricle is continuous. These minute vessels are constantly in action, so that their contents pass into the pulmonary veins during both the dilatation and contraction of the left auricle. The dilatation of this cavity removes what had accumulated during its contraction. The capillaries are unceasingly urging forward the blood, but as this can escape only at certain regular periods, it will accumulate during the intervals.

The blood in the *venæ cavæ* is placed precisely under similar circumstances, as will be shown in a subsequent part of this investigation. It is therefore evident that the condition of the circulating fluid in the pulmonary artery is very different from that in the pulmonary veins. In the one situation, it is immediately acted upon by the impulse of the left ventricle; in the other, it is removed to the farthest possible point from this source of motion, and is separated from it by an infinite number of minute vessels, which exert an important influence on the currents transmitted to them. These are striking differences, and prove

that the rate at which blood moves in the artery is greatly superior to that in the corresponding veins. It therefore necessarily follows, that the united capacity of the latter is greater than that of the former. Equality of capacity would be accompanied by equality of motion. It is unphilosophical to imagine, that the blood circulates in any vein at the same rate as in the artery from which it is immediately derived. A knowledge of the functions of both classes of vessels, and of their relations to the capillary system, is amply sufficient to refute such an opinion.

The capillaries are not simply carriers of blood. They are the seat of all vital changes, and microscopical examination shows that their slender currents, so far from being distinguished by any uniformly progressive motion, exhibit the greatest possible variety, nor are they immediately arrested by the cessation of the action of the heart. From these conditions, which universally belong to the capillaries, it is clear that the veins, into which they pour their contents, have a capacity much greater than the corresponding arteries.

The capillaries of the lungs have the same relations to the arteries and veins with which they are connected, as those of every part of the body have to the same classes of vessels, and hence the pulmonary veins have a capacity superior to the corresponding artery. This divides and subdivides into innumerable branches, terminating ultimately in capillaries; and the vessels which carry the oxygenated blood to the left side of the heart, gradually enlarge until they form the four capacious pulmonary veins.

The lungs are composed of three classes of vessels—the pulmonary artery and its subdivisions—the four pulmonary veins, and the infinite intermediate capillaries. Each contains a large quantity of blood, the circulation of which

will be differently influenced by the contractions of the right ventricle and the two acts of respiration.

The blood sent to and removed from the lungs, in any four consecutive contractions of the two ventricles, in the time of one inspiration, certainly does not prove that blood passes uninterruptedly during expiration from one side of the heart to the other. The portion of lungs nearest to the pulmonary artery is capable of containing several times the quantity transmitted by a single contraction of the right ventricle; and the portion nearest to the pulmonary veins possesses, also, sufficient blood to supply the left side of the heart, without that immediately propelled by the right. The contractions of the right ventricle transmit blood along the pulmonary artery and its principal divisions; but there is no evidence, nor is it probable that they urge it through the capillaries and larger vessels terminating in the four pulmonary veins.

When it is considered that the lungs are composed of minute vessels, exceeding all calculation in number, and possessing a contractile power, the blood in these veins can scarcely be imagined to be urged forward by the impulse of the right ventricle. Were this *vis-a-tergo* necessary, fatal accidents would frequently occur. In cases of severe congestion of the lungs, the blood circulating in the capillaries, the farthest removed from the right ventricle, cannot reasonably be imagined to be directly influenced by its contraction. The impulse exerted at this time is exceedingly slight; and the intermediate pulmonary tissue is not in a condition favourable to the transmission of it. A great part of the capillaries contain blood at rest, or possessing little or no progressive motion; and in this case how great must be the difficulties opposed to the transmission of the impulse, from this circumstance as well as from a diminution in the stimulating qualities of the blood—a

change which always accompanies this condition, of the lungs. The capillaries unquestionably possess a propelling force independent of the heart.

The lungs, as already stated, are liable occasionally to excessive congestion, amounting in fact to many times the quantity of blood usually contained in them, the greater part of which is stagnant, and often remains so for a considerable period. Is the equable distribution of it subsequently caused by the heart or by the capillaries? If the result be ascribed to the former, the opinion is fraught with insuperable difficulties. On this supposition, when the mass of blood to be moved is the greatest, it is nevertheless regarded as propelled into the veins by a force clearly inadequate to the effect. One of two conclusions inevitably flows from this doctrine. It must be admitted, either that the *whole* of the blood between the pulmonary artery and veins is so far influenced by the contractions of the right ventricle, as to furnish to the left auricle the requisite quantity, or that a portion only in the intermediate capillaries is acted upon. Were the former inference correct, congestion would be of short duration, and indeed it is questionable whether it could occur. What is understood by congestion is the accumulation of blood, the greater part of which is at rest. Now when the lungs are in this state, unaccompanied by acute inflammation, the contractions of the heart are feeble, and the pulse is usually small and weak, occasionally imperceptible. Is the slender stream of blood received at this time by the left auricle transmitted to it by the contractions of the right ventricle? According to this notion, when the mass of blood to be moved is the greatest, and the propulsive power the weakest, it is conceived to be pushed forward in its course, consequently, it is never stagnant in the lungs. This view is certainly untenable.

The left auricle is supplied with blood by the surviving contractions of a portion of the capillaries, which act when all other parts of the animal system have entirely ceased, and in ordinary cases of death it is usually found full of blood—a condition which cannot otherwise be satisfactorily explained. The last feeble contractions of the right ventricle will scarcely be looked upon as adequate to the effect. Were they the cause, the intermediate mass of capillaries ought to be in a state of congestion, to afford continuous columns for the transmission of the impulse. The pulmonary tissue is however exsanguineous and crepitous throughout.

In cases of sudden death from hanging—drowning, or from any circumstance suddenly arresting the respiration, there is a striking difference in the quantity of blood at both sides of the heart—a difference which would be calculated upon from the preceding remarks. The right auricle and ventricle, and the pulmonary artery, are generally very much distended, whilst the pulmonary veins and the left cavities are only moderately so, and occasionally only to a slight extent. The cavities of the heart contract some minutes after the lungs have ceased to receive air. The blood expelled by the right is immediately interrupted in its course towards the left cavities, hence congestion of the former as well as of the pulmonary artery. This effect would be anticipated from a knowledge of the causes in operation.

The pulmonary veins and the left cavities are very differently circumstanced during the gradual increase of the obstacles to the circulation; the left auricle and ventricle have not the same impediments as the right to the transmission of the vital fluid. This is not conveyed into capillaries, but into the aorta, consequently it is less liable to accumulate at the left than at the right side of the heart.

When both the right and left cavities have ceased to contract, the surviving activity of the capillaries still facilitates the flow of blood into the pulmonary veins and the left auricle, and causes in these a certain degree of distension. This varies considerably in amount in cases of the same kind of death, modified by the length of time the left cavities continue to contract.

Were the impulse of the right ventricle transmitted throughout the pulmonary tissue, the condition of the lungs would be different from what has been described. The congestion would not be confined to the right side of the heart; the lungs generally would be affected. The blood, it is contended, permeates these organs from the direct impulse of the right ventricle, until arrested in its progress either by chemical or mechanical obstacles. The former is the prevailing doctrine. The obstacles, according to this view, ought to commence at the farthest point from the impelling power, and therefore the congestion should begin at the left side of the heart and extend to the right. The latter, however, is chiefly the seat of it. If the circulation is arrested from a deficiency in the stimulating qualities of the blood, which is explicitly stated by modern writers, it ought unquestionably to be interrupted first in the capillaries the least under the influence of the right ventricle—the capillaries the farthest removed from it. It is a law of the animal system that congestion occurs most readily in vessels so circumstanced, and the case under consideration certainly forms no exception.

Sudden death, arising from any of the causes specified, does not give rise to congestion of the lungs, consequently two important inferences may be deduced: first,[¶] that the obstruction to pulmonary circulation is not simply chemical—and secondly, that the action of the heart is not transmitted throughout the pulmonary tissue. To establish

these points satisfactorily, it is necessary to adduce additional evidence, and this will be presented in a subsequent part of this inquiry. When asphyxia is the result of causes acting slowly, the lungs are always excessively engorged. In cases of this kind the obstacles to the capillary circulation are both chemical and mechanical.

To return to the subject under investigation, viz. : the influence of the heart on the motion of blood in the minute vessels of the lungs, it will scarcely be argued that in severe forms of congestion, the slender streamlets are urged into the left cavities by this power. It is indeed unequal to the effect. If a portion only of the capillaries be acted upon, a quantity of blood is acknowledged to be at rest. How is this subsequently put in motion? Certainly by the improved condition of the capillaries. The heart neither causes nor removes partial accumulations. The frequent expansion of the chest allows the small quantities of blood received to flow into the numerous capillaries, which being stimulated, carry forward their contents, communicating a slight motion to the blood in the pulmonary veins, the undue distension of which is gradually relieved by the contractions of the left ventricle.

Sir David Barry endeavours to illustrate the influence of inspiration on the motion of blood in the veins, by a diagram which, from its supposed accurate representation of important organs and functions in the animal system, is considered to demonstrate the fact. It will, however, on close examination, be found destitute of the conditions essential to a correct illustration of the phenomena. That the blood flows into the heart, as well as into the lungs, in the interval of two inspirations, will not be questioned, but in the instrument, the mercury or fluid employed, is said to rise only to the globe E, at times corresponding with

the vacuum made in it, so that in the interval of two vacuums the fluid is at rest.

Il est évident que le liquide contenu dans le tube flexible ne peut monter en E qu'à l'instant où le réservoir se distend pour former le vide, et qu'au moment où la tendance au vide cesse d'exister, le liquide obéira aux lois de la gravitation, et distendra les parties inférieures du tube.

Il est facile aussi de concevoir que les mouvemens du du liquide dans la branche A seront directement en rapport avec la force injectante, comme les mouvemens du liquide dans la branche B le seront avec l'expansion du réservoir E, et qu'une influence mutuelle sera exercée de part et d'autre en raison de la communication des deux branches en C, soit par un canal unique, soit par plusieurs canaux.*

This is not the condition of the blood in the interval of two inspirations. Its transmission into the right auricle does not depend upon, nor is it regulated by, the alternate expansion of the lungs. For his argument to possess the least force, the auricle should be supplied at times corresponding only with inspiration. He deduces the following conclusions from his experiments and reasoning, and regards them as satisfactorily established. It is obvious, however, that they are open to serious objections :—

D'après ce que nous venons d'exposer, on peut regarder comme prouvés les faits suivans :—

1° Que les cavités des grandes veines au-dedans du thorax et toutes les cavités thoraciques aspirent les fluides mis en communication avec elles.

2° Que cette aspiration n'a jamais lieu que pendant l'expansion des parois du thorax, c'est-à-dire pendant l'inspiration.

* Recherches expérimentales sur les causes du mouvement du sang dans les veines. Par David Barry, M.D. p. 46.

Desquels faits, et de ce que nous avons vu dans la dernière expérience, nous pouvons conclure,

1° Que *le sang qui coule contre sa propre gravité* n'arrive au cœur que pendant l'inspiration.

2° Que la principale puissance qui le pousse à travers les veines est la pression atmosphérique

3° Que, comme cette puissance ne peut être appliquée au sang des veines que pendant l'inspiration, ce sang doit nécessairement se mouvoir avec une rapidité qui est à celle du mouvement du sang dans les artères comme le temps occupé par une respiration entière est au temps d'une inspiration seule.

4° Comme le sang ne traverse les veines que pendant l'inspiration, et qu'il traverse sans cesse les artères, il suit qu'une accumulation doit se faire quelque part entre les deux ordres de vaisseaux et dans une quantité qui est à celle qui traverse les artères dans un temps entier de la respiration comme le temps de l'expiration est à la respiration entière.

5° Quant à l'accumulation qui doit être préparée pour l'aspiration du thorax, il importe peu qu'elle soit faite par deux pulsations de l'artère ou par six ; et par conséquent le fréquence du pouls ne peut être prise comme la mesure de vélocité du sang revenant au cœur : c'est la répétition des inspirations qui doit régler cette vélocité.

6° Il y a donc trois quantités de sang : une qui traverse l'aorte, une qui est aspirée par les veines à chaque expansion du thorax, et une troisième entre ces deux ordres de vaisseaux. Donc quand la respiration devient accélérée, cette troisième quantité est diminuée, et les deux autres augmentées en proportion ; mais, comme le cœur ne peut en admettre qu'une certaine quantité, les cavités aspirantes sont obligées de refouler le superflu pendant leur affaisse-

ment : de là des phénomènes pathologiques, dans la description desquels je n'entrerais pas à présent.*

In the instrument which he describes, it is evident that the motion of the fluid in the tube connected with the globe will not only depend on the vacuum which is made in the latter, but may indeed be measured by the frequency and completeness with which this takes place.† The fluid can escape only at this time. In the animal system the blood flows, however, into the right auricle, and from the right ventricle into the lungs, not at times corresponding with the dilatation of the chest, but even during its contraction. Here then is a striking difference between the instrument and the vital functions with which it is compared.

It is stated that the velocity of the blood returning to the heart cannot be measured by the frequency of the pulse, being in fact regulated by the amount of inspirations. In this remark there is a small portion of truth, but a much larger share of error, the exposition of which leads to some interesting physiological inquiries. One condition of the pulse may seem to afford no positive information respecting the rate of venous circulation, viz. a pulse weak and small, whether slow or frequent; but his objection is directed not against any particular kind of pulse, but against this generally as a measure of the velocity of venous circulation—a conclusion decidedly fallacious. Nor does the number of inspirations furnish any better data for determining the question.

The time required for the circulation of the whole mass of blood through any given point of the system depends on two circumstances;—the number, and the greater or less fullness of the contractions of the heart. If the blood be

* Opus cit. p. 37.

† See his work referred to, p. 41 to 49.

propelled by the left ventricle in a strong and bounding stream, the venous current will necessarily partake of the same character, and hence the rate at which the latter moves may be correctly inferred from the conditions of the former. The ease in which the contractions of the heart appear to furnish no satisfactory information is when they are small and frequent. At this time the venous circulation is exceedingly slow, and yet the arterial is accelerated. The frequency of the pulse under such circumstances may appear to be no indication of the rate at which blood moves in the veins, and yet a little consideration may show, that the information which it affords, philosophically studied, is not so destitute of practical value as a guide in reference to this subject, as imagined by the superficial reasoner. The phenomenon is not difficult of explanation.

When the powers of the animal system are greatly exhausted or depressed, there is a marked change not only in the qualities, but in the distribution of the blood. This leaves the extremities and surface of the body and accumulates in the internal organs, especially in the liver, the spleen, and the great veins. The contents of the arteries gradually diminish with the progress of this change, until at length the pulse becomes excessively small, feeble, and usually frequent. The quantity of blood in *actual* circulation is small, perhaps not above one-third or fourth what is ordinarily in motion. An extraordinary disproportion exists at this time between the contents of the arteries and veins. While the former are nearly empty, the latter are immensely distended. As the rate of motion in each class of vessels is in the inverse ratio of the amount of their contents, the venous current will be extremely slow, while the arterial is quick, estimating its rapidity by the frequent contractions of the heart.

A knowledge of these conditions of the circulatory system will enable the mind to form a pretty correct judgment of the rate at which blood moves in the veins, consequently, the pulse may be regarded, even at this time, a measure of the velocity with which the blood returns to the heart, or, in other words, it affords general data for such calculation. Barry asserts that the fact can be determined only by the number of inspirations. Were these to be our guide, they would certainly lead to conclusions as fallacious as the frequency of the pulse, considered independently of its other conditions. In those states of the animal system, co-existing with it, the breathing is invariably quick. Were the number of inspirations, therefore, a measure of the velocity of venous circulation, this would be regarded, but erroneously as accelerated.

It is impossible to entertain just views on the character of arterial or venous circulation, without understanding the general modifications in the distribution of the blood in connexion with every state of the pulse. The quantity in the arteries or veins, is not constant, but liable to important changes. At one time a great part of the vital fluid is at rest—withdrawn from the arterial system into the capillaries and veins, and yet the pulse will mostly be exceedingly quick; at another from exercise, or from whatever cause the bodily powers are excited, the whole mass of blood is in motion. The frequency and smallness of the pulse indicate one condition of the sanguiferous system: the frequency and fulness of the pulse another. To the philosophical inquirer they both present an accurate measure of the motion of venous blood.

Whenever the powers of life are suddenly exhausted or depressed, the inspirations are always frequent, and yet indisputable evidence shews that the venous blood is moving slowly. Its accumulation in the veins, and the

comparative emptiness of the arterics, establish the fact. The frequency of inspiration, without a correct apprehension of other conditions, to which Barry makes no allusion, does not furnish any measure of the velocity of venous circulation. Alone it is of the same value as the mere frequency of the pulse.

The instrument described by Barry cannot be viewed as conveying a correct idea of the influence of the thoracic organs on the motion of blood circulating against gravity. Poiseuille states he has proved, by direct experiments, that blood passes through the lungs when each side of the chest is freely opened, respiration being maintained artificially, and continues to circulate for at least an hour after the cessation of inflation. These experiments do not, however, authorize the inference which appears to flow from them, viz., that the expansion of the lungs is not at all essential to the passage of blood through them. They are too defective in several important respects to warrant this conclusion.

Inspiration accelerates the motion of the blood, only in so far as it diminishes impediments to its transmission from the right to the left cavities of the heart. The blood in the lungs and that in the *venæ cavæ* are so differently circumstanced, that either act of respiration will scarcely be imagined to affect both in the same manner. The striking dissimilarity in their conditions has not hitherto been taken into account, either by Barry or others engaged in the same train of inquiry. Inspiration occurring simultaneously with the dilatation of the right auricle will facilitate the flow of blood in the *venæ cavæ*, but to a very slight extent at any other time in ordinary breathing.

It may perhaps be objected, that the capacity of the auricle cannot justly be regarded as the measure or regulator of the influence of inspiration,—the space at the

termination of these vessels admitting of great enlargement, which will also give the blood a tendency to move in that direction, at the same moment that it flows into the auricle. Hence the capacity of the auricle, and the degree of enlargement of which that space is susceptible, might conjointly be regarded as the cause limiting the influence of inspiration. Were blood, however, transmitted at every successive expansion of the lungs to this space, this would soon become so occupied as not to allow of further increase. The constant circulation of blood, and the determination of a quantity at each inspiration to the roots of these vessels, would ultimately give rise to an immense accumulation.

Were this effect questionable, in the ordinary conditions of the system, it would be inevitable when these became excited or disturbed, and especially during the cruel tortures of experiments. The blood does not circulate against gravity in the *vena cava descendens*, consequently will flow towards the heart without the aid of the suction power of the chest. In the experiments of Barry, the blood in the *vena cava ascendens* was also necessarily in motion, from the great muscular efforts made by the animal. Admitting this fact, and the absorbing agency of inspiration, great accumulation would take place at the right side of the heart, and would arrest at once the farther agency of inspiration.

There is no continuous connection between the blood in the *venæ cavæ* and that in the lungs. Were it in the two situations united by an uninterrupted stream, a modification in the one would immediately be communicated to the other, but not otherwise. When the right auricle contracts, a barrier is of course placed between the blood sent out and that which flows in with the succeeding dilatation,

but the latter transmits no motion to that in the lungs. There are about three dilatations of the auricle, causing the blood to move in the veins, to one inspiration, hence this, as a moving power of the blood in these vessels, must be extremely slight. Poiseuille proved, by the most decisive experiments, that inspiration has no effect whatever on the motion of blood in the veins a short distance from the chest, and if these are punctured blood flows freely, though its passage to the heart be interrupted by ligature.

The experiments of Barry were ingenious, but from the unnatural and forced position in which the animal was placed, and the very great disturbance unavoidably produced in important functions, the laws of which it was the object of his researches to determine, the phenomena brought under observation cannot be received as illustrative of the normal influence of either act of respiration.

The instrument which he employed in his experiments was a spiral glass tube, at one end of which was attached an elastic and flexible tube which was introduced into the vein or pericardium; the other end was inserted into a coloured fluid. The ascent of this fluid in the glass tube, towards the chest, was regarded as evidence of the influence of inspiration—an illustration in fact of this influence on the course of venous blood, in the undisturbed conditions of the animal system. The stationary condition of the fluid, or its descent, was ascribed to the influence of expiration.

Just as the principle may appear in the conception and execution of this instrument, like many other attempts of the same kind to imitate the operations of nature, or to show by rude contrivances how they are carried on, it is liable to serious and unanswerable objections.

The horse was mostly the subject of experiment. It was thrown upon the ground and held down against the violent

efforts which it would occasionally make to rise. The respiration would thereby be extremely hurried, and the circulation necessarily equally disordered. It was under these circumstances that the coloured fluid in the spiral glass tube introduced into the large veins, was observed to ascend towards the chest on inspiration.

The manner in which these experiments were performed might possibly induce a relation between the blood in the lungs, and that in the great veins, which does not naturally exist, evolving phenomena which cannot be regarded as demonstrative of what occurs in the ordinary actions of the animal system. The blood in the *venæ cavæ*, as already remarked, has no continuous connection with that in the lungs, and therefore how greatly soever its motion may be accelerated in the latter, by inspiration, the contents of these veins will not be at all directly influenced by it. When an animal is tortured, as the horse was in this instance, it is not improbable that the right auricle and ventricle may be so affected that the dilatation of the former may take place, when the tricuspid valves are only imperfectly closed by the contraction of the ventricle, and thus a partially uninterrupted current may connect the lungs and the *venæ cavæ*. Were this the case, the deep and violent inspirations would unquestionably draw the blood from these veins, or the coloured fluid in the spiral tube in connection with them towards the chest, no obstacle being opposed to the limited exercise of such power. The change in the natural action of the heart would produce this result, and the probability of its occurrence is strengthened by the fact, that it was not observed when the experiments were made upon the animal standing.

“Que, quand l'animal était debout, la pression atmosphérique n'était jamais si fortement marquée que quand il

était renversé. Je m'en assurai en répétant l'expérience sur le même animal dans les deux positions, et je vis en effet que la coïncidence des mouvemens du liquide avec la respiration ne pouvait guère s'observer dans la première position, parce que la respiration du cheval debout et en repos est presque insensible."*

Admitting the truth of this explanation, what will be the condition of the veins immediately at their termination, after two or three violent inspirations? If blood flow abundantly to them from the diminution of pressure, will not considerable accumulation be the inevitable result, arresting the further agency of inspiration? It will scarcely be urged, that the accelerated contractions of the heart prevent the effect; for though strong and frequent there are formidable difficulties to the passage of the blood through the lungs, arising from the disturbed breathing and constrained position of the animal. That they do not is evident from the swollen and pulsating jugular veins.

It is evident, from the further experiments of Barry, that he had not considered either the conditions induced in the venous system, by his interference, or the relations between this system and the circulation of blood in the lungs.

In illustration of this fact, the spiral tube, which had previously been placed in the veins, to exhibit the influence of inspiration, was subsequently introduced into the pericardium, and the same result was observed. How differently, however, is the fluid circumstanced in the two situations. From what has been already stated of the relation which the blood in the *venæ cavæ* bears to that in the lungs, it is clear that the introduction of a tube into the pericardium cannot in any degree exemplify the agency

of inspiration on venous circulation. The greatest possible difference exists in the two cases. The pericardium and the *vena cava* have not the same relation to the heart. The coloured fluid in the spiral tube introduced into the former, will certainly enter with more or less facility according to the pressure made upon the sac; but the effect, whether great or only just perceptible, is no evidence of the influence of inspiration on venous circulation. To compare phenomena occurring in such dissimilar circumstances, shows how little the relations between different parts of the circulatory system have been studied.

The next step is to examine the influence of respiration on the motion of the blood in the lungs. That the expansion of these organs, whether natural or artificially produced, is essential to the free transmission of blood through them, will scarcely admit of doubt. It is difficult, however, to ascertain the precise influence which it exerts. Does it give to the blood propelled by the right ventricle additional velocity, from exerting a power of suction, or does it only remove obstacles to its flow into the minute capillaries? If pulmonary circulation depended on the former cause, it would be arrested on the cessation of it. Poiseuille has shown, however, that when two incisions are made into the chest, securing by artificial means the occasional expansion of the lungs, circulation for some time is nevertheless maintained. It is stated for at least an hour after the discontinuance of inflation.

“ On fixe un tube dans la trachée-artère d'un chien, et au moment où on ouvre largement les deux côtés du thorax parallèlement au sternum, on pratique à l'aide d'un soufflet la respiration artificielle. A peine la poitrine est-elle ouverte, l'air entre dans sa cavité, le poumon est déprimé sur les côtés de la colonne vertébrale en vertu de son

élasticité ; on pousse l'air dans le poulmon, les cellules se dilatent, et par suite tout l'organe : par conséquent la pression de l'air contenu alors dans le poulmon l'emporte sur celle de l'air ambiant. Quand on cesse de souffler, le poulmon revient sur lui-même par son élasticité ; l'air qu'il contient a encore une pression supérieure à celle atmosphérique. Dans cette expérience, qui d'ailleurs n'est pas nouvelle, toujours l'air contenu dans le poulmon a une pression plus grande que celle de l'atmosphère ; il n'y a plus d'aspiration du sang veineux, et cependant la circulation continue très-bien à se faire, tellement que l'animal vécut encore pendant une heure au moins, temps après lequel, fatigué de souffler, on s'arrêta."*

This experiment certainly does not afford results from which any positive conclusions can be drawn. It is scarcely possible to find in the whole range of experimental physiology an instance in which the important functions of life are more violently disturbed than on this occasion. No organ retains its natural conditions. The trachea is divided to receive a tube—two large incisions are made into the chest, and then respiration is carried on artificially. The nice adjustments of the animal system cannot exist under such circumstances, nor can any inference be deduced with respect to the influence of any vital power. Truth and order cannot be evolved out of such general derangement.

It is stated that pulmonary circulation continued not only during this state of things, but even after the cessation of inflation, and indeed when the lungs had ceased to act. In evidence of this we are told that the animal lived an hour. If the functions of life were carried on so admirably,

* Journal de Physiologie Experimentale et Pathologique, par F. Magendie. Tome x. An. 1830, p. 290.

why did it die so soon? The animal could not have lived this length of time unless the lungs had received air, and though the quantity might be small, nevertheless sufficient to aërate the feeble currents in motion. Had the blood flowed in quantity and force through these organs, as in health, or in any degree approximating to it, the conclusions of Poiscuille would then have been just and legitimate.

The experiment cannot be performed without producing an important result, to which allusion is not made, viz. congestion of the lungs. This condition invariably follows the division of the trachea, and especially when respiration is attempted to be carried on artificially. Every such interference with this cardinal function has in all my experiments been characterized by this effect. It is this circumstance which prevents the elasticity of the lungs acting as in health, causing a complete collapse, and it is impossible that anything like a normal state of the pulmonary circulation can co-exist with it.

It is stated that the lungs, after the cessation of inflation, are superior, from the air which they contain, to the pressure of the atmosphere. It is not, however, the small quantity of air which prevents the subsidence of them, and secures the continuance of the circulation. This is a misconception. The cause which counteracts the external pressure is the congestion of these organs—a condition much more conducive to the continuance of pulmonary circulation than their apparently complete collapse. The abundance of accumulated blood furnishes a trickling from various channels in the direction of the left cavities of the heart, and life ceases only when these streamlets are arrested. If this explanation be correct, the experiment is evidently of no value in this investigation. If it proves anything it proves a great deal too much.

When the lungs are expanded by inspiration, the pulmonary artery and its numerous subdivisions are in a condition favourable for the reception and transmission of blood, but in the succeeding expiration they are compressed, and partial congestion is produced in that portion of the lungs directly connected with them. This appears to be proved by several phenomena. When the mind is strongly interested in the relation of a story, or by any powerful appeal to its feelings, the two acts of respiration are not performed in their regular succession, but, on the contrary, a sigh or deep inspiration is made at long intervals. This arises from the accumulation of blood in the vessels, and in that portion of the lungs nearest to the right side of the heart. The prolonged inspiration is proportionate to the extent of the congestion, which it is necessary to remove. In all diseases of the pulmonary organs, in which a similar condition exists, the breathing is short and frequent, and at times marked by deep inspirations.

Greater the congestion, or less able the lungs are to receive and aerate the blood, and the more frequent is respiration. The capillaries cannot, as under ordinary circumstances, circulate the several quantities propelled by the contractions of the right ventricle, unless the inspirations be accelerated to relieve the congestion on the side of the lungs nearest this ventricle, which, however is often only partially effected, as is evident from the pulsation of the jugular veins, arising from the difficulties to the transmission of blood through the lungs, whether from recent congestion, tubercular disease, or from any other cause.

Violent exertion, as running for example, changes the natural character of respiration. During the almost suspended breathing at the beginning, the flow of blood through the lungs is very much impeded. On the cessa-

tion of the exercise, the inspirations are greatly accelerated in order to facilitate the transmission of the several quantities of blood previously sent out by the right ventricle. The balance of circulation would be restored were the exercise long persevered in, as is strikingly exemplified in persons practised in running. At first they are distressed, breathing with difficulty ; but so far from this increasing, the respiration after a time gradually becomes less laborious, they having arrived at what is commonly called the second breath. The hurried respiration arises principally from the cause assigned,—more blood being conveyed by the right than is removed by the left ventricle, the diminished air in the chest creating difficulties to its free transmission. The left ventricle, however, is soon vigorously roused and removes, in a series of contractions, a greater quantity of blood than is sent to the lungs by the corresponding contractions of the right, so that the balance of circulation previously broken, is again re-established, and continues from the equally excited action of both sides of the heart.

Bourdon, who has studied this subject with attention, offers the subjoined explanation, which is partly corroborative of the foregoing remarks.

“ La compression qu'éprouvent les poumons se fait sentir sur les vaisseaux à sang rouge aussi-bien que sur les vaisseaux à sang noir ; sur les radicules des veines pulmonaires comme sur les divisions de l'artère du même nom, Or, voici ce qui résulte de cette compression des vaisseaux à sang rouge des poumons :

“ Dans le premier instant, plus de sang afflue vers les cavités gauches du cœur, qui pour cela redoublent d'activité. Mais si l'effort persiste, le sang des cavités gauches du cœur et des artères diminue, à proportion que celui des

cavités droites et des veines augmente. Les faits suivans le démontrent :

1°. J'ai souvent tâté le pouls de personnes affectées de coqueluche, d'asthme, ou même de catarrhes pulmonaires : chaque fois qu'il survenait de fortes quintes de toux, je sentais plus de force et plus de *fréquence* dans les battemens de l'artère ; en même temps la face était plus colorée : voilà pour le premier stade. Si la quinte continuait, le pouls devenait petit, *irrégulier*, quelquefois même à peine sensible, et la face violacée. Enfin, la quinte diminuait et cessait, et le pouls et la face reprenaient, l'un, sa fréquence et sa force ; l'autre, sa couleur habituelle.

“ J'ai fait des observations semblables dans les grands efforts de l'accouchement et du vomissement : dans tous ces cas, le trouble survenu dans les battemens des artères coïncidait toujours avec les changemens de couleur de la face ; et il en devait être ainsi, puisque ces deux phénomènes prennent leur source dans la même compression des poumons.”*

The smallness and the irregularity of the pulse on the long continuance of cough, in the diseases which he mentions, does not prove that the blood at the left side of the heart diminishes in proportion as that at the right increases, or that these peculiarities of circulation arise from deficiency of blood at the left side of the heart. The pulse is almost invariably small and feeble, and generally frequent in congestion of the lungs, unaccompanied by acute inflammation, and, so far from these conditions being attributable to too little, *they are mostly the effects of, or co-exist with, too much blood.* The undue quantity determined to the chest

* Recherches sur le Mécanisme de la Respiration, et sur la Circulation du Sang, Par Isid. Bourdon, p. 70-71.

does not undergo its ordinary chemical changes, in consequence of which it is incapable of exciting strongly the left side of the heart.

The views which are here advanced respecting the influence of expiration are opposed to the prevailing doctrines of physiologists. Experiments cannot, in this case, be appealed to, in order to settle the question. The experiments which have been employed have interfered too directly with the operations of nature, and have induced too great a disturbance in the functions of the chest and of the circulatory system, to furnish clear and unexceptionable results. The phenomena brought forward by writers in confirmation of the truth of their opinions are by no means satisfactory. Haller remarks “in expiratione sanguis in pulmones compressos difficiliter recipitur, difficiliter adeo cor dextrum se deplet, stagnat sanguis in venis cavis, in vena jugulari, in cerebro demum toto, atque adeo sinus cerebri, et venæ jugulares intumescunt, quod a minimis vasculis sanguinem accipere pergant, emittere nequeant.

“Ad hanc causam altera accedit, quam non monitus per experimenta cognovi,* ornatus vero ad phænomenon interpretandum adhibuit Franciscus Lamure.† In expiratione nempe inprimis thorax contrahitur, comprimuntur pulmones, auriculæ, *venæ cavae*, fit refluxus sanguinis in venas cerebri, hæ ergo in expiratione turgent, cerebrumque una totum. Hinc presso, ut ego presseram, thorace,‡ clisique *vena cava*§ perinde cerebrum elevatur.

“Dudum, si placeret malignum esse, J. Adrianus Slevogt|| in tussi cerebri sinus intumescere vidit, emetica¶

* Second mem. sur les part. sens et irrit. Exp. 76.

† Pp. 547, 558, 559, 562, &c. ‡ P. 548. § Pp. 551, 552.

|| In disp. de dura mater.

¶ G. v. Swienten Comment. T. iii. p. 266.

vero medicamenta, quæ expirationem violentam cient, sanguinem in caput determinare alii Cl. viri. Verum omnino uberius hæc, eum experimentis conjunxit Franciscus Lamure, et etiam mea experimenta* non sinunt dubitare, quin veram phænomeni causam aperuerit. Videntur hæc amplius a Cl. de Bordeu diduci, ut etiam pulsum arteriæ a brachio refluum cum inspiratione, undamque quasi expulsum cum tussi sive expiratione, conjungi narret."†

The phenomena adduced as evidence that the blood circulates through the lungs during inspiration only, will not, on careful examination, be found to establish the fact. That the blood enters these organs with difficulty, after several violent expirations, is admitted; but there is a marked difference between the obstacles opposed to its entrance, and the condition of that already in motion towards the left side of the heart. Obvious and important as this distinction must appear, it is one that has been overlooked. The phenomena adduced by Haller and other authorities prove, that the blood enters the lungs with difficulty during a series of expirations, but do not afford any conclusive argument respecting that portion in circulation towards the left auricle. The subsidence of the lungs cannot possibly influence, in the same manner, the blood which enters and that which leaves these organs, and yet an influence, the same in degree and kind, is contended for, or implied in the arguments advanced. It is not to be supposed that the capillary circulation of the lungs is arrested by expiration. The pressure is too partial to produce this effect. An obstacle is certainly opposed to the free transmission of blood along the pulmonary artery and its immediate

* Second mem. Exp. 116, 117.

† Du pouls, p. 324. *Elementa Physiologiæ, Tomus secundus*, p. 335.

subdivisions, so that, after an uninterrupted series of expirations, the blood must necessarily meet with considerable difficulty in its passage from the right ventricle. Congestion will take place on the side of the lungs nearest this cavity, the blood not being able to escape either by a retrograde or progressive motion. How differently is it circumstanced in the direction of the left side of the heart! Pressure in this case will urge forward the blood, and especially during the dilatation of the left auricle, which in ordinary breathing occurs about three times to one expiration.

There are other opinions entertained by high physiological authorities concerning the influence of expiration, which are very questionable, if not, indeed, entirely destitute of truth. Burdach states, that the aortic system receives less blood during inspiration than expiration.

“ Le système aortique reçoit moins de sang pendant l'inspiration, et davantage pendant l'expiration. Poiseuille* a trouvé le courant artériel diminué dans le premier cas, et augmenté dans le second. Lorsque Bichat† avait ouvert la carotide d'un animal, et que celui-ci venait à érier ou à faire une grande expiration, le sang jaillissait avec plus de force. Quand il respirait par la bouche, en saignant du nez, Bourdon‡ rendait, dans l'espace de trente secondes, dix à douze gouttes de sang pendant l'inspiration, et quinze à seize pendant l'expiration ; une inspiration prolongée faisait cesser le saignement. Des hémorragies apaisées, à la suite d'une amputation, reparaissent quand le malade tousse, et il arrive quelquefois aux anévrysmes de se rompre pendant une forte expiration. Je ne puis déterminer le

* Répertoire général d'anatomie, T. vi, p. 70.

† Recherches sur la vie et la mort, p. 223.

‡ Rech. sur le mécanisme de la respiration et de la circulation, page 77.

moindre changement dans mon pouls par l'expiration la plus longue ; mais il disparaît complètement lorsque je fais une inspiration soutenue."*

Before proceeding to analyse these facts, and the reasoning which they have suggested, it may be well to present the following passage from Bichat in confirmation of the same views. He had studied the phenomena with attention, but found it impossible to offer a satisfactory explanation of them.

“ D'ailleurs, l'influence des grandes expirations sur la force de projection du sang par le cœur est très-manifeste, sans toucher à la trachée-artère. Ouvrez la carotide ; précipitez la respiration en faisant beaucoup souffrir l'animal (car j'ai constamment observé que toute douleur subite apporte tout-à-coup ce changement dans l'action du diaphragme et des intercostaux) ; précipitez, dis-je, la respiration, et vous verrez alors le jet du sang augmenter manifestement. Vous pourrez même souvent produire artificiellement cette augmentation, en comprimant avec force et d'une manière subite les parois pectorales. Ces expériences réussissent surtout sur les animaux déjà affaiblis par la perte d'une certaine quantité de sang ; elles sont moins apparentes sur ceux pris avant cette circonstance.

“ Pourquoi, dans l'état ordinaire, les grandes expirations faites volontairement ne rendent-elles pas le pouls plus fort, puisque dans les expériences elles augmentent très-souvent le jet du sang ? J'en ignore la raison.”†

The doctrine which is here taught and generally received by writers, may certainly be shown to be erroneous, and the fact which Bichat acknowledges his inability to explain, will, on a little consideration, be found to present no diffi-

* *Traité de Physiologie*, Tome vii, p. 48. Paris, 1837.

† *Anatomic générale*, Tome i, p. 157.

culty. Expiration can exert no *direct* influence on the quantity of blood sent into the aorta. The immediate changes produced by it in the condition of the circulation in the lungs, cannot in any degree affect the amount of fluid transmitted by the contractions of the left ventricle. Admitting that it increases the flow of blood towards the left cavities of the heart, the left auricle, if dilating at the same instant of time, receives the quantity so determined, and the next moment, when the ventricle dilates, and subsequently contracts to convey this into the aortic system, expiration, even if it take place simultaneously with the latter action, can manifestly have no direct agency on the strength of the current expelled.

As the dilatations of the ventricle are in the proportion of about three to one expiration, this will be as likely to occur when the ventricle is dilating as contracting, and if during its dilatation, it will not be contended that the arterial system receives at this time a strong current of blood, when indeed none is transmitted. But even supposing expiration and the contraction of the cavity to take place at the same instant, these two powers are evidently not acting on the same column of blood. The one is sending fluid, either into the left auricle or in the direction of it, the other into the aorta.

The phenomena which physiologists bring forward in support of these views may be readily explained on other principles. The effects of expiration on the arterial current are entirely attributable to the contraction of various important muscles. The number and force with which they are brought into play depends on the degree of bodily exertion induced. If great, the arterial current will be projected with violence; if slight, with a proportionate diminution of strength. It is also easy to explain why

severe voluntary expirations do not at once give additional strength to the pulse, while violent expirations in experiments cause the blood to be emitted with great energy from divided arteries.

The pulse depends on the contraction of the left ventricle—the accelerated flow of the arterial blood in the experiments of Bichat, on the pressure which is made on the arteries. The two causes have not the slightest connection. Strong voluntary expirations cannot give rise to the same obvious results as occur in experiments on divided or bleeding arteries. *Unless the blood can escape into day from the pressure*, the influence of expiration cannot possibly manifest itself. This cannot either accelerate or strengthen directly the pulse, its varying conditions depending on corresponding changes in the contractions of the left ventricle.

A little further consideration will illustrate the striking difference in the circumstances of the blood in experiments in which the arteries are divided, and in the undisturbed conditions of the animal system. In the former, it has a tendency, to a certain degree independently of the heart, to flow in the direction of the least pressure, consequently the quantity which escapes from a divided artery, in a given time, is far greater than would have passed through any point of the vessel in a normal state. Violent expirations—which are strong muscular contractions acting on some parts of the sanguiferous system—will necessarily cause the blood to flow with increased energy towards the divided artery. The same phenomenon would occur in any series of inorganic and elastic vessels, presenting open mouths, on the application of fits of external pressure. In the normal condition of the animal system, how is it possible for expirations to modify *directly* either the number or force of the pulse when this is produced by the contraction of the left ventricle?

The blood is unquestionably influenced in its motion both in veins and arteries on which the muscular contractions immediately act, but the extent to which this occurs does not show itself obviously to the senses; nor is the circulation by any means so disturbed as in the case of divided arteries. Hence the difficulty which Bichat experienced, in studying the phenomena, is at once solved in taking into consideration the difference in the condition of the blood in the natural and induced circumstances of the arteries.

Burdach, further maintains that less blood enters the lungs during expiration than during inspiration. The facts in favour of it are imagined to be numerous and irrefragable, and are thus marshalled by the learned physiologist :

“ Pendant l'expiration il pénètre moins de sang dans les poulmons, et les veines caves éprouvent une tuméfaction qui se propage tantôt plus et tantôt moins à leurs branches. Il suffit déjà d'une expiration médiocre pour voir les veines jugulaires se gonfler chez les personnes maigres; chez celles qui crient, rient ou toussent avec force, le gonflement s'étend à toutes les veines de la tête, et même en partie à celles du reste du corps; Bourdon* l'a vu, chez des animaux, se propager jusqu'aux veines crurales et mésentériques; il a remarqué aussi quelquefois† que les veines caves se tuméfiaient, sur les Chiens, pendant l'aboïement. La réplétion outre mesure du système de la veine cave tient à ce qu'il se vide moins dans les poulmons, une partie du sang y refluant même du cœur droit. Haller‡ a vu ce liquide rétrograder, pendant l'expiration, dans la veine cave supérieure jusqu'au cou, et dans l'inférieure jusqu'au foie.

* Sur la respiration et la circulation, p. 68. † Ibid., p. 65.

‡ Element. Physiolog. t. iii, p. 137, 141, 203.

Cotugno* dit même avoir observé, dans les sinus veineux du cerveau, des pulsations isochrones, à celles des artères, et il croit que l'oreillette pulmonaire, plus remplie de sang pendant l'expiration, repousse le tubercule de Lower (§ 708,1) dans l'oreillette droite, de manière que le sang de la veine cave supérieure est obligé de refluer vers la tête, tandis que celui de l'inférieure s'épanche plus librement dans le cœur."†

These phenomena do not establish the fact contended for. It is necessary to make a distinction between the effects of ordinary, and a continued series of extraordinary expirations. A difference in this respect makes them distinct causes in operation. The quantity of blood which enters the lungs, is not in any way modified by ordinary expirations. These have as little influence upon it, as upon the quantity of blood expelled by the left ventricle. The same reasoning applies to both cases, and with equal force. Indeed they are analogous, with this difference, that in the one, the contraction of the chest acts upon arteries, and in the other upon veins.

No continuous connexion existing between the column of blood in the *venæ cavæ* and that flowing into the pulmonary artery, a modification in the one cannot possibly influence the amount sent out by the right ventricle. Were the relation between the two of the nature of an uninterrupted column, this effect might take place, but not otherwise. The retrograde motion of blood occasionally observed in the *venæ cavæ*, during expiration, is no evidence of any deficiency of fluid at the right ventricle, nor of the existence of any impediments to its passage through the lungs, though certainly it is most apparent when such are present. The pheno-

* Giornale per servire alla storia della medicina, t. vii, p. 176.

† Traité de Physiologie, tome vii. p. 45.

menon frequently occurs when it cannot be attributed to this cause. In thin and delicate individuals, one or two expirations, as in coughing, will often be seen to produce it, in which case it cannot be ascribed to the compression of the lungs preventing the ingress of blood. The contents of the right ventricle are not so immediately arrested in their course as to occasion such result, consequently the auricle has no difficulty in relieving itself: *it has, however, during a lengthened series of strong expirations.*

The regurgitation is regarded as indicating the existence of impediments either in the heart or lungs, when it may be shown to originate in the altered condition of the great veins. In delicate and debilitated constitutions, a relatively increased proportion of the vital fluid accumulates in the venous system, causing considerable distension of the veins near the heart, extending at times to the jugulars. This condition rarely exists without being accompanied by pulsation in the latter; but this is not to be traced to any difficulty experienced by the right auricle in transmitting forward its contents, but to two circumstances—the contraction of the chest, and that of the auricle. The effect is rendered manifest in consequence of the large column of blood extending from the heart to the point where it becomes apparent. To diminish the column, without in any degree modifying the condition of the lungs, would be to prevent the manifestation of the effect.

The influence of inspiration on pulmonary circulation is limited, in my view, to the removal of impediments to the passage of blood through the lungs, and, therefore, it is not a matter of surprise, that it was not arrested in the experiments of Poiseuille, in which artificial inflation was employed; nor is the continuance of it difficult of explanation, when inflation had been interrupted for an hour. The

feeble circulation carried on at this time can in no degree elucidate the ordinary effects of inspiration.

Voluntary and deep inspirations corroborate the views which are here proposed in reference to their natural operation. When the inspirations are forced and frequent, in place of being in the ratio of one to about three dilatations of the right auricle and ventricle, they occur perhaps simultaneously with the dilatations of the latter. This will, of course, facilitate the flow of blood into the auricle, and also its passage from the ventricle into the lungs, but not equally its transmission through them. Pulmonary congestion is the consequence, if such inspirations be continued for only a very short period. In trials upon myself, acute pain was caused in the chest; the pulse was accelerated, but diminished in strength. In what manner, however, can we explain on these views, persons diving and remaining under water a considerable time, without any injurious effects? What would be the condition of the sanguiferous system, agreeably to the doctrine that blood circulates only in the veins during inspiration? The motion of the blood in these vessels would be arrested, and the larger arteries would be almost emptied, receiving scarcely any supply from the lungs. These results would be incompatible with the safety or well-being of life. The objection is not equally strong against the views advocated in these pages. Before diving, the chest is distended by a full inspiration, and as the breath is held, it remains in this state, hence the blood is enabled to enter and pass through the lungs sufficiently freely to prevent any serious derangement of the vital powers. This view is amply borne out by experiments which were performed by Bichat to determine the difference in the colour of the blood, on arresting the

breathing after an inspiration and expiration. The arterial fluid presented its natural appearance much longer after the former than the latter, which fact proves, that pulmonary circulation was much longer maintained after the one act than the other.

BOOK II.

THE PROPERTIES AND INFLUENCE OF ARTERIES ON THE CIRCULATION OF THE BLOOD.

It would be a vain and unprofitable undertaking to endeavour, by an appeal to experiments, to reconcile the discrepant doctrines of physiologists on the organisation and properties of the arteries. The results obtained by different able inquirers present scarcely any points of agreement. It is asserted by one that the arteries are muscular and possess considerable contractility; and the opinion is stated to be confirmed by an analysis of their structure, as well as by the effects observed on the application of external agents. The mind, in this case, is not called upon to believe anything but what is presented to the senses. Conclusive, however, as the evidence may appear, it is treated by another as a fiction—an ingenious assumption to account for phenomena which may otherwise be explained. He equally appeals to experiments, and courts the examination of the impartial understanding. These, it is argued, prove the arteries to be neither muscular nor contractile, but simply endowed with elasticity. The mechanical and chemical agents, which the one urges

in support of his views, are stated, by another, to be incapable of causing the several effects contended for. Indeed, the same experiments do not, in different hands, give rise to the same effects. A third physiologist, enlightened also by experimental researches, simple and easy of application, denies altogether the exercise or modifying influence of either contractility or elasticity on the motion of blood.

My business is not to endeavour to reconcile these discordant conclusions, but to examine the numerous facts brought forward, bearing upon or illustrating the phenomena of circulation. The investigation is not biassed by any preconceived opinions, nor have I any particular views to establish, except what arise strictly from the analysis of the subject. A patient consideration of the labours of past and contemporary physiologists will be far more useful than any attempt to emulate them by the introduction of new facts, except such as may be elicited by a critical study of those with which the physiologist is familiar. The demand for bold and novel experiments is less urgent than a just appreciation of the nature of the teeming results, theoretical and practical, which crowd upon and perplex the inquirer. To facilitate their better understanding is the object of these researches.

Magendie, in entering upon the consideration of the properties and functions of the arteries, remarks, "we have applied ourselves chiefly to study the various circumstances under which the elasticity of the arterial parities is developed, and have shown how this physical property is intimately connected with the most important condition of the circulation in the human body, There is not, in fact, a single phenomenon of any consequence which may not be explained by reference to this principle, and to this only. The motion of the blood through the different tubes,

whether arterial or venous; its rapidity in the different sections of each system; the perfect regularity of its progress; the jerking stream which flows from a divided artery;—all these are circumstances, the explanation of which is to be sought for in the physical properties of the vessels themselves, and in the mechanical nature of the force which gives the blood its first impulse.”*

According to some writers, elasticity simply allows an artery to return to its previous state on the cessation of the distending cause; consequently, reaction is proportionate to the tension which precedes it. This definition appears philosophical and just. In this restricted sense, however, it is not received by others. To this property Magendie ascribes extraordinary influence. He views it as capable of expelling the whole contents of an artery; and experiments are adduced in proof of the fact. The arterial system is regarded as always full of blood, and therefore, the additional quantity transmitted by the contraction of the left ventricle necessarily produces distension. In the interval of two contractions, the arteries act upon their contents, in virtue, it is said by one authority, of elasticity, and by another, of muscular contractility. Numerous experiments and phenomena are imagined to demonstrate the existence and influence of the former property, but which may be shown to be much less conclusive than is generally supposed. Bichat and Parry state distinctly that no dilatation of the arterial system was detected on the contraction of the left ventricle, and even Magendie, the great advocate of elasticity, acknowledges that dilatation is perceived only in the aorta.

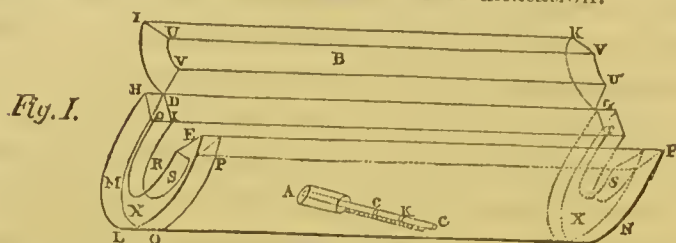
Poiseuille is considered to have established the dilatation of arteries, and the exact force which they exert in returning

* The Lancet, Dec. 20, 1834, page 476.

upon their contents, and consequently to have thrown considerable light on the phenomena of circulation. His experiments are referred to, as affording decisive evidence of the truth of his opinions. To doubt the accuracy of the results which they furnish, may appear presumptuous. There is, indeed, much to admire in the conception and execution of them; but this excellence must not blind the understanding to glaring defects;—to serious errors arising from the application of the principles of physical science to the investigation of vital phenomena.

Some of the most distinguished physiologists of the present day will unquestionably retard the sound progress of the study of man, by the constant recourse to experiments, and the doctrine which they inculcate, that the animal machine is comparatively simple, and its important laws explicable on purely mechanical principles. At the head of those who enforce this opinion and whose labours have been directed to establish the correctness of it—stands Magendie. His intelligence, enterprise and tact, give him a marked pre-eminence, but he has travelled at too great a speed, and over too ample a space to have been able to note or examine fully what was most interesting in his path of inquiry. His progress exhibits considerable industry and varied researches, but few elevations affording a clear and comprehensive view of the vast field of vital actions. The justness of these strictures will be established by a critical analysis of the most important of his contributions to physiology. The experiments of Poiscuille were mostly performed under his superintendence, and furnished the same results when subsequently repeated by him; and therefore, they come before the world invested in all the imposing qualities of his admitted ability and high reputation.

Figure I, represents the instrument employed by Poiseuille to establish the existence of dilatation.



"M N is a tin tube of two decimetres in length, about 8 inches, and thirty-five millimetres in diameter, about 1.37 inches. On its convex surface is contrived an aperture, D E F G, occupying its whole length, which can be closed by a sort of door D G K I. The extremities M and N, (see M in the figure,) present grooves formed by the two plates H L E R Q, and D O P S T, about one centimetre, 39-100ths of an inch, distant from each other, and so fluted in their centre as to exhibit a segment of a circle of twelve millimetres, = 4-700ths of an inch, in diameter. The door I D G K supports two plates, I D V U, K G U' V', whose margins V U, V' U' are cut circularly, so that when closed, they meet to form entire circles with the corresponding segments F X' S. In the middle of this door B, appears a circular orifice of two centimetres, or 7-110ths of an inch in diameter, the use of which will presently be apparent.

"At a point A in the surface of the cylinder is an opening of twenty millimetres (78-100ths of an inch) in diameter, into which is fitted a plug of cork, receiving a small glass tube, of three millimetres (11-100ths of an inch) internal diameter. A scale divided into millimetres (39-1000ths of an inch) is here fixed. This tube is nearly horizontal.

"The trunk of the carotid of a horse is exposed to the extent of 11½ inches; ligatures being applied to each of the branches arising from it for the purpose of completely

isolating the artery. The cylinder is opened, and the exposed portion of the artery placed in it, which, however, still adheres by its extremities to the animal. The door I D G K is closed, and there is dropped into each of the grooves a mixture of suet and wax ; the joints of the door are luted ; and thus the cavity of the cylinder has no communication with the outside, excepting by the aperture B, and the glass tube A C. By this orifice B, some water at about 36° is introduced. A part of this water enters the tube A C, say as far as C'; the cylinder being now filled with water, and no longer containing any air, the orifice B is closed by a plug. Everything being thus arranged, the artery is encased in the cylinder, and the blood moves in its interior, *as in the ordinary state*. If the tube A C be examined, the water is seen to change its level, and to move from C' to K', and *vice versa*, and that too at each contraction of the heart. In the experiment now detailed, the distance between the points C' and K' was 2.7 inches ; thus the artery being 35-100ths of an inch in diameter, its 8 inches of length, in consequence of their dilatation from each contraction of the heart, presented an increase of volume equal to that of a solid cylinder, whose height was C' K, and the diameter of whose base was that of the small tube A C, viz., three millimetres, (11-100ths of an inch). We may notice that this dilatation is far from being considerable, and not easily recognised by a single observation, even in an artery of the calibre of that experimented on, after being exposed."*

Poiseulle remarks that in this experiment "the artery is encased in the cylinder, and the blood moves in its interior as in the ordinary state."

* Journal de Physiologie, tome ix, p. 46.

Were arteries inorganic tubes, his reasoning might be admitted, but certainly not otherwise. He presents no facts elucidating the natural condition of these vessels, but evidently imagines that they are unsusceptible of serious modifications, and unaffected by experimental operations. His deductions are founded upon this assumption, and in this lies the great error of his ingeniously contrived experiments. The constrained position and suffering of the animal would greatly alter the conditions of the circulation. The heart would be excited to violent action, and the fluid propelled by it would meet with impediments in its course, which would necessarily produce distension of the artery; and moreover, the effect would be aggravated by the application of ligatures to the branches which it gives off. The blood being confined within narrower limits would cause great fulness of the vessel. But, independently of the influence of the latter circumstance, the suffering and position of the animal are quite sufficient to occasion an unusual degree of distension, not only in the vessel in question, but throughout the whole arterial system. If the intention of the physiologist had been to produce this effect, or the widest possible departure from the natural conditions of circulation, he could not have succeeded more admirably. With what truth can it therefore be asserted that blood circulates in the artery as under *ordinary* circumstances? Whenever the respiration is accelerated and disturbed, and especially in investigations of this kind, extensive derangement of the circulatory powers is an inevitable result. What, then, is the legitimate inference deducible from the experiment? That an artery, already round and full, is susceptible of further enlargement? The dispute is not whether arteries have a varying capacity, at one time small, at another large and

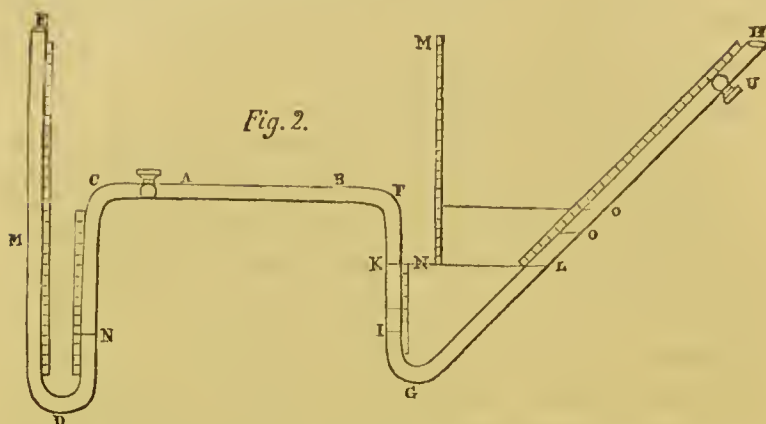
full to distension, but whether they are dilated in the *ordinary* conditions of the circulation and to what degree. The experiment furnishes no data to direct and elucidate this inquiry. Without suspecting the possibility of error in these researches he concludes :—

“ Les artères se dilatent donc dans la circulation ; mais si elles augmentent de volume, leurs parois sont distendues ; comme leur tunique moyenne est éminemment élastique, en revenant sur elles-mêmes elles donnent naissance à une force ; c'est cette force de contraction toute passive, due à l'élasticité des parois artérielles, mise en jeu par leur dilatation, que nous nous proposons maintenant de mesurer.”*

In the experiments of Barry, the coloured fluid in the instrument employed was observed to ascend towards the chest during inspiration, but when performed on the horse standing, in place of being held down forcibly, this effect was scarcely perceptible. In treating of this subject it was remarked, that phenomena, the result of experiments which create an unnatural condition of the circulation, cannot certainly be regarded as elucidating the laws by which this important function is carried on, in the undisturbed state of the vital powers. The same remark, may with equal force, be applied to the experiments of Poiseuille. The means employed to determine the elastic properties of arteries, and their influence on the motion of the blood, to be of any value, should in no way disturb the natural condition of these vessels. To induce a change is, in all probability to bring into play causes not previously in operation, and consequently to give rise to effects which may greatly mislead the inquirer. This truth will be obvious in studying the attempts of Poiseuille to measure the elastic force of arteries, or their influence on circulation, and to these

* Journal de Physiologie, tome ix, p. 48.

attempts, which are thus minutely described by him, especial attention is solieited.



" A, B, (Fig. 2,) is an arterial tube 250* millimetres ($9\frac{1}{2}$ inches) in length, belonging to the trunk of the earotid, which has just been detached from a living horse; its diameter is nine millimetres ($35\text{-}100$ ths of an inch); it has horizontal position, and its extremities are tied on to the horizontal branches A C, and B F, of the two glass tubes A C D E, B F G H, whose branches E D, C D, and F G, are vertical. The branch G H is inclined, and forms with the [vertical one an angle of nearly 50 degrees; in the portion K G L of the tube F, K G H, there is a quantity of quicksilver, the levels of which are K and L. The portion L H is filled with water, and there is a stop-cock at H, which is elosed after the introduction of the water from the level of the mereury at L to the extremity H. This tube B F G H has a small diameter, say of three millimetres ($11\text{-}100$ ths of an inch). From K towards G there is a scale, the amount of whose divisions correspond with the millimetres of a third vertical scale M' N'. The portion

* The length of the tube was measured in the living animal before being detached.

B F K of the tube B F G H, the artery A B, and the tube A C D E, (which has a stop-cock at A,) are filled with water. A quantity of mercury being introduced at the orifice E, the water pushed by the mercury from D C A mounts into the artery A B, and communicates to the yielding arterial parietes a pressure increasing in proportion to the quantity of mercury introduced. When a quantity of this metal is introduced sufficient to produce the pressure we wish to obtain, the stop-cock at A is shut, and thus all communication between the artery A B, and the tube A C D E is intercepted. By means of the vertical scales placed along the branches D E and C D, we can calculate the pressure to which the parietes of the artery are submitted. M and N are the levels of the mercury in these two branches. In the present experiment, the difference of the levels is equal to 85 millimetres ($3\frac{25}{100}$ inches); the portion E M, which is filled with water, to 278 millimetres, and C N, also filled with water, to 148 millimetres: we have then the force which dilates the artery expressed by the height of a column of mercury equal to

$$\begin{array}{cccccccc} \text{mill.} & \text{mill.} & \text{mill.} & \text{mill.} & \text{mill.} & \text{mill.} & \text{mill.} & \text{mill.} \\ & 278 & - & 148 & & & 130 & \\ 85 + \frac{\quad}{13} & - & = & 85 + \frac{\quad}{13} & = & 85 + 10 & = & 95 \text{ (the densi-} \end{array}$$

ties of the water and mercury which we employed were to each other as 1 to 13).

“ The stop-cock at H is then opened, and the artery by virtue of its elasticity, suddenly recoils upon itself; the mercury is depressed from K to I, and then rises from L to O, and as we have K I = 63,25 mill.; K F = 50 mill.; P N = 39, mill.; P M = 216,12 mill.; there is obtained the force of contraction of the artery represented by the

$$\begin{array}{ccccccc}
 & \text{mill.} & & \text{mill.} & & \text{mill.} & \\
 \text{height of a column of mercury equal to } 63,25 \text{ mill.} + 39 + & & & & & & \\
 216,12 - (50 + 63,25) & & & & & 102,87 & \\
 \hline
 & 13 & & & & 13 & \\
 7,91 \text{ mill.} = 110,16. & & & & & &
 \end{array}$$

“ During the whole experiment we must take care to keep the artery in a warm medium, by means of water at 36 degrees. (Cent. = 97° F.) On comparing the present result with the preceding, we find, that the entirely passive contraction of the artery, in consequence of the elasticity of its parietes, brought into play by the dilatation is superior to the force which dilates it.”*

This experiment is supposed to prove that the elasticity of an artery is even superior to the force by which it is brought into play, and, moreover, that it co-operates powerfully with the heart in the propulsion of blood. The experiment will be admitted to establish the existence of elasticity, but not the degree of influence which it exerts on the motion of blood. The two questions are very distinct. The one is simple and easy of solution, the other is complex and exceedingly abstruse. No experiment can be devised to illustrate more satisfactorily the operation of elasticity, but to measure the agency of it in the natural conditions of the circulation is beyond the resources of science. If the blood be admitted to move in arteries in virtue of elasticity and the impulse of the heart, in any experiment to ascertain to what extent the former power co-operates, it is imperatively necessary not to disturb the latter. They have relations to each other which must be scrupulously preserved. The arterial fluid would not be at rest in the interval of the contractions of the heart were elasticity altogether wanting. There are powers

* Journal de Physiologie, tome ix. p. 48.

by which it would be solicited or urged along its course, hence the difficulty of determining the exact influence of elasticity on the circulation. When this property is imagined to be exerted, the blood is in motion, but in the experiment the fluid employed is at rest. To render this or any experiment of value, the distension of the arterial system in its ordinary condition ought to be known as a preliminary step, but unfortunately there are no data to guide the physiologist in such investigation. In the experiment, the artery is distended, but whether to a greater or less extent than natural, is entirely conjecture. The result cannot, therefore, convey a correct idea of the influence of elasticity on the motion of blood. Its existence is shown, but not the exercise of it in its normal circumstances.

The analysis of these experiments will not be uninteresting, even if the justness of the strictures be disputed. The difficulties inherent in physiological researches are pointed out, and especially when important organs are injured, or the vital actions of the animal system are disturbed. Experiments cannot dispense with the necessity of deep and laborious thought, or the greatest possible caution in reasoning upon the phenomena brought under observation. This appealing directly but rudely to nature is often a fruitful source of error.

Before proceeding further, it is necessary to place before the mind, in few words, the objections to these and all similar researches.

The experiments of Poiseuille have two objects in view. The one is to demonstrate the existence of elasticity—the other to measure its influence. The first experiment is supposed to set at rest the question of dilatation; but in what circumstances of the arterial system?

1st. The artery is in a state of distension, and every fresh quantity of blood transmitted by the left ventricle will increase it. The fluid injected, in pushing the mass before it, will unquestionably produce this effect, and such would occur, were a vein similarly treated. But it is not possible to place both vessels in analogous conditions.

2ndly. The arterial fluid is admitted to be in constant motion. Were the heart the only propelling power, the blood would not be at rest after every impulse of the left ventricle. It would proceed in its course, and, how trifling soever in degree, the consequence would be a contraction of the artery, or at least a diminution of its calibre. It is difficult to determine whether this phenomenon is the result of an active vital property in the vessel, or arises from the continued motion of the blood *after* the contraction of the ventricle. The same effect would be exhibited by the first instrument in either case; the artery would be distended by the next quantity of blood injected. In analysing the experiments of Magendie bearing on this matter, it will be shown that he, also, entirely neglects this distinction.

3rdly. It is admitted that the dilatation of the artery is demonstrated by the experiment. But this might have been inferred from a knowledge of the circumstances in which the vessel is placed. A vein, were it possible to subject it to analogous conditions, would present the same phenomenon, and therefore the experiment affords no information respecting the influence of elasticity on the arterial current.

4thly. The object of the second experiment is to measure the influence of elasticity on the motion of blood. There are no data on which to proceed in the inquiry. The first step is one of conjecture. The distension of an

artery, in its ordinary condition, is constantly varying, and at no one moment can it be ascertained. It is an unknown quantity defying all analysis.

5thly. The experiment proves that an artery when greatly distended, as in this instance, is capable of re-acting on its contents, but no conclusion can be drawn from the fact in reference to the agency of elasticity in the circulation of the blood. It is an assumption to assert that this property maintains the same intensity of motion throughout every part of the arterial system. The doctrine will, in subsequent investigations, be shown to be fallacious.

It is now necessary to pass to the examination of the experiments of Magendie, which are adverted to by all physiological authorities, as establishing the vast influence of elasticity on the motion of blood.

“ Quand deux ligatures sont appliquées en même temps, et à quelques centimètres de distance, sur deux points d’une artère qui ne fournit pas de branches, telle que la carotide, on a une longueur d’artère dans laquelle le sang n’est plus soumis, qu’à la seule influence des parois. Si l’on fait à cette portion de vaisseau une petite ouverture, presque tout le sang qu’elle contenait est aussitôt lancé au dehors, et l’artère se rétrécit beaucoup. Cette expérience, qui est connue depuis longtemps, réussit constamment. Je ne sais sur quel fondement quelques écrivains ont pu mettre en doute sa réalité.”*

Is the blood, in this experiment, ejected from every particle being in motion, or from the action of the artery?

The fluid, included by the ligatures, was, previously to their application, flowing from the impulse of the heart, and

* Journal de Physiologie Expérimentale, tome i., page 109.

though arrested in its course, the activity of it cannot at once be suspended. Were the upper ligature removed, the blood would proceed in its natural direction, *and puncturing the vessel merely allows this tendency to circulate or escape to manifest itself*. The structure of an artery does not allow it to contract with force upon its contents. The effect is an impossibility. Were the elasticity of the arterial parietes the cause of the phenomenon, and the only one in operation, its influence would be much more evident than it is in the ordinary conditions of the circulating system.

The force with which blood flows from a punctured vein might with equal truth be enlisted in corroboration of the same doctrine. The projection of the fluid in this case would arise, neither from the action of the vessels nor the impulse of the heart; nor is there any evidence that the phenomenon in the experiment of Magendie is attributable to the former of these two powers. Did the atmosphere offer the same amount of resistance to the arterial fluid escaping from its vessels, as is experienced by it from the mass of blood before it, no jet would be perceived, nor would the contents of the included portion of the artery be forcibly expelled. To render the experiment satisfactory, the resistance should be equal in both cases. Were it possible to place the artery in *vacuo*, the ejection of the fluid would be still greater, but clearly not from the increased action of the arterial parietes; yet this inference might be drawn, as in the experiment in question, and certainly with equal propriety. The effect is obvious,—the escape of the fluid; but the cause has been assumed without sufficient consideration of the difficulties which it involves. Magendie adduces another experiment, and one which is peculiarly his own, in support of the doctrine which is here combated.

“Voici une autre expérience qui me paraît propre à mettre dans tout son jour le phénomène de la contraction des artères. J’ai mis à découvert l’artère et la veine crurales d’un chien dans une certaine étendue; j’ai passé au-dessous de ces vaisseaux près du tronc, un lien que j’ai ensuite serré fortement à la partie postérieure de la cuisse, de manière que tout le sang artériel arrivât au membre par l’artère crurale, et que tout le sang veineux retournât au tronc par la veine crurale. J’ai appliqué alors, une ligature sur l’artère, et en quelques instans, le vaisseau s’est vidé complètement dans la partie placée au-dessous de la ligature.

“ Il est donc évident que la force avec laquelle les artères reviennent sur elles mêmes est bien suffisante pour expulser le sang qu’elles contiennent.”*

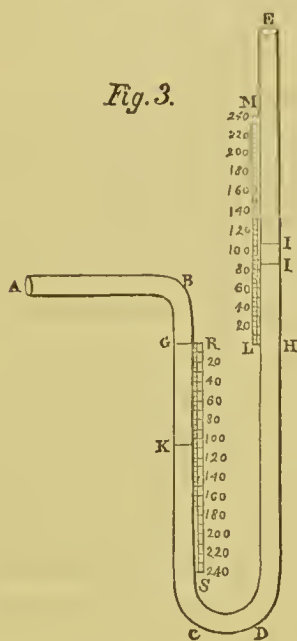
In what manner is the blood circumstanced in the artery previously to the application of the ligature? Is it at rest, or in motion? If in motion, which it is unquestionably, how is it affected by the ligature? This cannot subtract from it the impulses of the heart which it has already received, and therefore it continues to flow towards the capillaries until they have received the whole contents of the artery; so that the vacuity of this vessel does not arise from the exercise of contractility, but from the tendency of the blood to pursue its course, partly in virtue of the propulsive power previously imparted to it. Were contractility the cause, as Magendie and others contend, the artery would not be entirely emptied. Its partial contraction would expel only a small portion, and not the whole of its contents. This fact ought to have awakened suspicion as to the correctness of the view taken.

* Journal de Physiologie Expérimentale, tome i., page 109.

The same physiologist observes, that if a ligature be tied on the carotid artery, the blood above it is wholly expelled; or, if on a vein, the blood below it, or nearest the heart, circulates and leaves the vessel empty. Previously to the application of the ligature, the blood in the vein is similarly circumstanced to that in the artery, it is in motion, and whether from a *vis-a-tergo*, or causes acting before it, is immaterial to the argument. The ligature cannot arrest this motion in either vessel. The capillaries, acting in advance of the arterial current, will tend to withdraw the blood from the artery. The tying of a ligature will indeed influence the current: whether an artery or vein be the subject of experiment, the fluid will be urged forward by it. When two ligatures are applied, if the latter be so placed, which it may readily, as to distend the included portion of vessels, the contents will necessarily be projected with accelerated force, and certainly not from any increased contractility being brought into play, but simply from the greater tendency of the blood to escape from the pressure of the ligature. These experiments are constantly appealed to in proof of the influence of elasticity on the motion of blood. They cannot, however, if these strictures are well founded, be admitted as establishing the fact.

Poiseuille has endeavoured to determine, by an ingeniously suggested instrument, the force with which the blood circulates, which has always been regarded as diminishing gradually with the onward progress of the fluid. All physiological authorities were unanimous in this opinion, and indeed it appeared to rest on self-evident data. Poiseuille has, however, recently attempted to prove that the blood is urged forward with as great a momentum in a small artery far from the heart, as in any important branch near it. This extraordinary doctrine comes before us in an exceed-

ingly plausible form, established by direct experiments and which have been repeated by Magendie with corresponding results. The experiments, however, may be shown not to warrant the conclusions deduced from them. They are thus described by Poiseuille:—



“Figure 3 is a glass-tube presenting a horizontal branch, A B ; a vertical descending branch, B C ; and an ascending branch, D E ; so curved as to exhibit at B the fourth of a circle, and at C D a semicircle. Supposing mercury to be introduced into the portion G C D H, the tube being in a vertical position, the levels of the mercury G and H will be at the same height in both the branches. If the blood enter into the portion A B G by the orifice A, inosculated with an

artery, it will press on the surface G of the mercury ; the metal will be depressed in the branch B C from G to K, for instance, while it will rise in the branch D E to I. It is evident that, in accordance with hydrostatic laws, the whole force with which the blood throws itself into the artery will be estimated by the weight of a cylinder of mercury, the base of which is a circle, having for its diameter that of the artery, and whose height is the difference G K of the two levels of mercury, deducting the height of the small column of mercury, which may form an equipoise to the column of blood B K.

After this description of the instrument, he proceeds to show how it acts in the experiments in question.

“Meantime let the apparatus be fitted to the carotid artery of a dog; the distance $B G$, which measures the height of the sub-carbonate of soda above the level of the mercury must be determined. Let us suppose, for greater simplicity, that the portions $G K C$ and $H J' I$ of the tube are exactly of the same diameter, so that, on observing on one and the same scale, $L M$, the height $H I$ to which the mercury has risen, we shall see that it sunk below the point G of the same quantity $H I$. Thus, in order to find the height of the mercury, owing to the force of the blood, we shall only have to double the height $H I$, and to subtract from this result the pressure of the mixture of blood and sub-carbonate of soda, owing to the column $B G K = B G + G K = B G + H I$.

“Let us take, then, $B G = 25$ millimetres, nearly one inch, and $H I = 105$ millimetres, or four inches. Let us suppose again, as we have found, that a column of the mixture of blood and sub-carbonate of soda of 10 millimetres, or four-tenths of an inch in height, furnishes an equipoise to a height of one millimetre of mercury, we shall have for the pressure sought,

$$105 + 105 - \frac{25 + 105}{10} = 210 - \frac{130}{10} = 210 - 13 = 197 \text{ mill.}$$

and for a height $H I'$, equal to 85 millimetres, or 3.34 inches, for instance the pressure we should have would be

$$85 + 85 - \frac{25 + 85}{10} = 170 - \frac{110}{10} = 170 - 11 = 159 \text{ mil.}$$

On seeking the means of these two pressures we should have

$$\frac{197 + 159}{2} = \frac{356}{2} = 178 \text{ mill.}$$

We shall be enabled to arrive at the expression of this mean by the following arrangement:—

The highest point.	The lowest point.	1st mixture of blood & subcarb. of soda.	2d mixture of blood & subcarb. of soda.
105 + 105	85 + 85	25 + 105	25 + 85
Sum, 380 millimetres.		Sum, 240 millimetres.	
Mean pressure, 380 — $\frac{240}{10} = \frac{356}{2} = 178$			

The principle which he deduces from these experiments is thus stated by him : “ From the identity of these results with the preceding, we may irrevocably conclude, that the force with which a molecule of blood moves, be it in the carotid or aorta, &c., is in all respects equal to that which moves a molecule in the smallest arterial branch ;* or, in other words, that a molecule of blood moves with the same force in the whole tract of the arterial system ; which *a priori* we with all other physiologists were far from thinking to be the case.”† ‡

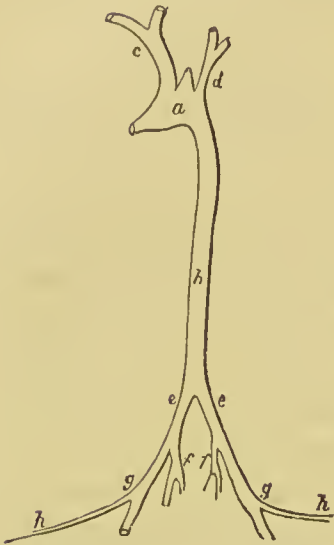
The blood in the experiment, is not placed under circumstances analogous to its condition in the animal system. It is clearly confined to the space A B G, and is wholly at rest in the interval of the contractions of the heart. The arterial fluid is acknowledged to be in constant motion, consequently the two cases are exceedingly dissimilar. The fallacies involved in these experiments will be best

* Bichat, in his “ Anatomie Générale,” on the limits of the heart’s action denies this equality of force in the trunks, branches, and ramuscles ; but, notwithstanding the respect due to the opinion of so great a physiologist, we find ourselves under the necessity of entertaining the opposite view.

† When we say that this force is the same in the whole tract of the arterial system, we do not mean to reject the modifications which it must experience in certain points which present a special arrangement, as the anastomotic arches of the arteries of the mesentery, the arterial circle of Willis, &c.

‡ Journal de Physiologie, tome viii.

Fig 4



exposed by the diagram. Fig. 4; *a*, arch of the aorta; *b*, descending aorta; *c*, trunk of the carotid artery; *d*, the left subclavian artery; *e, e*, the common iliac arteries; *f, f*, the internal iliac arteries; *g, g*, the external iliac arteries; *h, h*, the epigastric arteries.

Poiseuille, in common with all physiologists, regards the circulatory system as full, so that every successive quantity propelled by the left ventricle will produce two effects,—arte-

rial distension, and the forward motion of the column of blood. Were one instrument to be fixed in the trunk of the carotid artery, and another in one of the epigastric arteries, reasoning *a priori*, Poiseuille observes, that it would be expected that the blood of the former artery, from its nearness to the heart, would act upon the mercury with greater force than that of the latter, which is not the case, nor ought such to be the result. The instrument at *c* is certainly nearer the heart than the one at *h*, but as the blood in each artery, and that occupying the space A B G, in both instruments, cannot flow forward in the intervals of ventricular contraction, it must be considered as a column extending to the heart, the force of which will necessarily be exerted equally on the contents of each artery and corresponding instrument. The same impulse being impressed on each column, the instrument will inevitably furnish corresponding results. The propulsive power is the same, and if dissimilar effects were

produced, these would be referable to a difference in the length of each column acted upon, and not to the blood becoming slower as it proceeds in its course, which Poiseuille imagined might cause the instruments to indicate different degrees of pressure. This condition is annihilated by the experiment, and consequently it is vain to expect the indication of a power which the researches destroy.

These remarks are sufficient to show that the conditions of the circulation are greatly disturbed by the application of the instrument, and therefore the effects observed do not warrant the conclusions drawn from them. The instrument is perhaps better calculated to determine the amount of the propulsive power of the heart than any that has yet been devised ; *but its fitness for this purpose renders it altogether inapplicable as a measure of the force by which the blood is moved in different portions of the arterial system.* The two questions are very distinct. The one is comparatively simple,—the other is exceedingly complex. The blood in the artery and that in the space A B G of the instrument, which forms part of this column, is not in motion in the interval of the contractions of the heart, and cannot even, when acted upon by them, move forward beyond a few lines, hence the pressure exerted on the mercury is clearly a measure of the contractile power of the left ventricle. The force with which the blood circulates in different parts of the arterial system cannot be determined by any instrument which arrests the motion of the fluid.

The equal momentum of the blood throughout the body is regarded by Poiseuille, not as produced by the heart, but by the elasticity of the arteries, which comes into play immediately after the contraction of the ventricle. This secondary power, however, can operate only when the fluid

on which the arteries act is enabled to flow forward, which is impossible in these experiments. The mercury ascends in the systole of the heart, but in the succeeding diastole, when elasticity is supposed to be exerted, it does not ascend still higher, but falls ; which would not be the case, according to the views of Poiseuille, were its condition analogous to that of the blood in the arterial system. This physiologist has endeavoured to prove, as already stated, that the elasticity of the arteries is even greater than the force by which it is excited. How does it happen then that the mercury ascends only at times corresponding with the contraction of the left ventricle, and invariably falls with its succeeding dilatation ? If the two forces were equal and consecutively brought into play, this effect would not be observed. The height to which the mercury ascends, by the contraction of the ventricle, ought at least to be maintained if not exceeded by the elasticity of the arteries, according to the doctrine here insisted upon. If the arteries exerted the power which he states, this instrument ought to afford some indication of it, but none whatever is perceived. The manner in which this equal momentum is imagined to be maintained throughout the body is thus explained by him :

“ Lorsque le cœur se contracte, une ondée de sang est poussée dans le système artériel déjà plein de sang ; les phénomènes qui suivent immédiatement la projection de cette ondée sont *la dilatation des artères, une sorte de locomotion de tout le système artériel* par laquelle les courbures tendent à se redresser. Ces phénomènes ne peuvent avoir lieu qu’aux dépens de la force avec laquelle l’ondée est lancée par le cœur ; mais à peine le système artériel s’est-il dilaté, à peine ses-courbures ont-elles cédé à l’action du cœur, qu’en vertu de l’élasticité du système

artériel toutes les artères revenant sur elles-mêmes, rendent à la force du sang tout ce qu'elle venait de perdre, et, par suite, la force avec laquelle l'ondée de sang est lancée du cœur, conserve la même intensité jusqu'aux dernières ramifications artérielles, ainsi que nos expériences l'ont constaté."*

It is not possible to appeal to experiments in evidence of the truth of this doctrine, for they annihilate the very powers they are proposed to measure. To arrest the motion of blood in an artery, in order to determine the force with which it moves in its ordinary conditions, is a new mode of philosophizing. It would be quite as correct to interrupt the course of a stream until it overflowed its banks, to ascertain the strength of the current when unimpeded; yet such is the method of investigation adopted by ingenious men familiar with physical science.

Hales, in his experiments on the motion of blood in the arteries, observed that the blood rose and fell in the tube connected with them at times corresponding with the contractions and dilatations of the left ventricle. The blood in the tube, and the mercury in the instrument of Poiseuille, have precisely the same relations to the heart. Each becomes a continuous column extending to this organ, and is urged forward in the same manner by its direct impulse. In neither case can the elasticity of the arteries be brought into play. If the doctrine of Poiseuille were well-founded, would not the blood issuing from arteries in different situations of the body be ejected with equal force, every particle possessing the same intensity of motion? This, however is not the case. "As the distance from the centre increases, the circulation slackens from several causes, and the blood could not reach all the parts of the

* Journal de Physiologie, tome viii., pp. 296, 297.

body, if the arteries, whose vitality increases with their distance from the heart, and as they become smaller, did not propel it to all the organs. A very remarkable difference is observable between the blood which is sent to the *mammæ*, as I have several times noticed in removing the carious bones of the toes, or in extirpating eancerous breasts. The small arteries of these parts are nearly of the same size, but the jet of blood is sent to a much greater distance when one of the mammary arteries is divided."* The blood in these arteries is not at all impeded in its course, as in the experiments of Poiseuille, consequently the character of the stream emitted may be regarding as indicating the difference in the force with which it is propelled. Poiseuille contends that this difference is attributable to the influence of respiration. In his experiments, however, neither the respiration nor the circulation is natural, and therefore it is unphilosophical to compare phenomena evolved at this time, with the undisturbed operations of the system. The breathing, from the constrained position and suffering of the animal, becomes frequent and violent, and the arterial system generally distended, hence these important functions have relations to each other very unlike what ordinarily exist.

The stream emitted during surgical operations conveys a much juster idea of the force with which the blood is moved, than can be derived from the elaborate experiments of this physiologist. Its escape in the one case, is not at all interrupted, therefore it is strong or feeble, according as it is near to or remote from the heart. In the other, as already remarked, it is confined to a tube, and its tendency to flow forward is arrested by a column of mercury. The

*Elements of Physiology, by A. Richerand, translated by G. J. M. De Lays, M.D.

important changes, produced by tedious and painful experiments on the vital functions, are not only frequently overlooked, but are extremely difficult of calculation. The truth of this is forcibly exemplified by the experiments of Poiseuille. He attempts to prove that the blood is circulating with equal force throughout the arterial system, from the contraction of the left ventricle and the elasticity of the arteries. The latter cause is regarded as exerting the same degree of influence as the first, and as their action is alternate, the blood is necessarily in constant motion. According to this view, the blood injected into the aorta by the contraction of the left ventricle acts upon a column already moving rapidly forward, and not upon a column at rest. The same impulse impressed on two equal columns, the one in motion, the other at rest, would produce very different results. It is comparatively easy to estimate the force necessary to propel the one, *but it is impossible to calculate the powers employed in moving the other*. An appeal to physical science will be in vain, for there is nothing strictly analogous in the whole range of mechanical operations.

The blood in the arteries circulates principally from the combined influence of respiration, muscular contractions, the action of the capillaries, the accumulated impulses of the left ventricle, and to a slight extent from the elasticity of the vessels themselves. The position in which the animal was placed in the experiments of Poiseuille, conjoined with its sufferings, would disturb to a serious extent the ordinary conditions of the arterial fluid, and thus affect the accuracy of the results obtained. All obstacles to its regular and uninterrupted flow will unquestionably destroy the influence of three of these powers—the agency of the capillaries, the elasticity of the

arteries, and the impulses of previous ventricular contractions, so that the column in the experiments of Poiseuille is acted upon chiefly by the heart. If two instruments, therefore, be applied to arteries differently situated in respect of this organ, they will indicate the same pressure, but clearly from the annihilation of powers which produce important modifications in the character of the circulation.

This temporary and unnatural condition of the arteries, is what Bichat imagined to exist at all times. He looked upon the arteries as exerting no influence whatever on the motion of blood, nor did he believe in the causes usually assigned as retarding it, such as its passage into vessels, the conjoint area of which increases as they subdivide, friction, angles and anastomoses. His experiments in illustration of his views are liable to the same objections as those of Poiseuille. They prevent the operation of powers which it is his object to analyse. The following were imagined by Bichat to be peculiarly favourable to his doctrine, and in connexion with this inquiry are worthy of consideration :—

“ 5°. La force du cœur fait circuler le sang par des tuyaux inertes, adaptés aux artères dans un trajet très-considérable. Si on coupe un pouce de l’artère carotide, et qu’on substitue un tuyau engagé dans les deux bouts ouverts de cette artère, le sang traversera ce tuyau, et la fera battre comme à l’ordinaire au-dessus. Je ne puis concevoir ce qui a pu en imposer à ceux qui ont obtenu des résultats différens.”

7°. Adaptez un tube à une artère, et qu’à l’autre extrémité de ce tube il y ait une poche quelconque de peau, de taffetas gommé, et le sang la remplira aussitôt ; puis à chaque contraction de cœur, elle vous présentera une espèce de battement. C’est ainsi que bat la tumeur anévrysmale,

quoiqu'étant cellulaire. Quel que fût l'organe qui concourût à former le kyste, celui-ci battrait de même pourvu qu'il recût par le sang l'impulsion du cœur."*

In the first experiment the blood continued to circulate, and pulsations were felt beyond the insertion of the tube, hence the inference that the heart was the only propulsive agent. In the second, the pouch was quickly filled with blood, and exhibited pulsations synchronous with the contractions of the left ventricle ; an effect which might have been anticipated. It is manifest that, in both cases, but especially in the latter, all motific powers, except that of the heart, were destroyed, consequently no positive deduction can be drawn from them. There is, indeed, the same objection to all the experiments of Bichat to determine the causes which move the blood. The removal of a portion of the carotid artery and the substitution of a tube, in the manner stated, is an operation both tedious and painful. It cannot possibly be performed without producing great disturbance in the ordinary conditions of the circulation. The arteries, as already remarked, invariably become distended under such circumstances, so that a series of inert tubes would at that time carry on the circulation as well as organic vessels.

The second experiment presents a still wider departure from these conditions. In this the blood is confined to the artery and tube, and has no means of escape, or progression beyond the pouch, so that every contraction of the left ventricle must necessarily be communicated to the latter, with the same force and regularity as if the artery were metallic in its structure. Had the objections to these experiments been fully understood, succeeding physiologists would have avoided falling into similar errors in their reasoning.

* Anatomie Générale. Système Vasculaire à sang rouge.

A recent writer, of considerable acuteness, has endeavoured to show that the arteries are in no degree dilated by the contraction of the left ventricle. He remarks:—

“It has been supposed that the circular fibres of the ARTERIES were muscular, and that they contracted and relaxed at each pulse, and that the throb felt was caused by a dilatation of the artery; those fibres are NOT MUSCULAR, but more approaching to a ligamentous tissue, firm and, though elastic, not yielding to the force of the heart, but, on the contrary, preserving the CALIBRE of the artery UNIFORM, as may be seen by laying bare an artery in a living animal, or when the artery is laid bare in an operation; it is LONGITUDINALLY that the arteries are STRETCHED at each injection from the heart, by which their capacity is increased, the consequence of which, from their being bound down in various places, is, that there is a SERPENTINE motion in the artery where it is at all loose.

“The fibres of the middle coat of the artery being arranged circularly, allow of the separation laterally, and thus *accommodate* themselves to the *elongation*, of the tube, whilst they *resist* its *dilatation*. Now it may be thought that the motion of the arteries *seen* at the wrist and in the temples is their dilatation, but it is the serpentine movement caused by the alteration of the curve, the artery being elongated at each injection from the heart.

“Where the artery is perfectly straight you may lay it bare and scarcely see it move, but the moment you compress it with the finger, or tie a ligature around it, you perceive it pushed at every pulse. To illustrate the deception of the sensation which the pulse gives, as if the artery were dilated at each beat, if a long vein removed from the body have a syringe adapted to one end, the other being raised, or arranged with spring valve, which yields to the

jets so as to keep it full, and fluid be sent through in jets, it will, upon pressure by the finger, give the sensation of dilatation, but the eye perceives none : again, if any one grasp the leather tube of a fire or garden engine, the sensation given will be that of its expanding in the hand at each stroke of the pump, but the eye contradicts the sensation, *it is merely the tendency to resume the cylindrical form from the outward pressure of the fluid, but not expansion.*"*

It is here stated that the circular fibres preserve the CALIBRE of the artery UNIFORM. It is unquestionably UNIFORM, as far as the eye can discover, in the circumstances in which it is examined. He speaks of the condition of the vessel when laid bare in a living animal, or when exposed in an operation. In both cases, the arterial system is always in a state of plethora. It cannot possibly be otherwise. The mental sufferings, both in the experiment and the operation, rouse the action of the heart, augmenting both the force and number of its contractions; and, moreover, the blood in the arteries laid bare, from the contraction of the muscles in the vicinity of them, and from other causes is impeded in its flow, in consequence of which these vessels will be distended. They are always, at this time, found round and resisting to the touch, which is clearly not their ordinary condition. The circulation, in either case, is nearly as much disturbed as in the experiments of Bichat, nor do the phenomena at this time throw any light whatever on the functions of the arteries. The writer observes that it is LONGITUDINALLY that they are STRETCHED at each injection from the heart. Such, indeed, will be the appearance, and, to a certain extent the effect in the

* First Principles of Medicine. By Archibald Billing, M.D., A.M., 2nd Edit. p 10.

unnatural state in which they were placed. Being full to distention, every fresh quantity of blood transmitted by the left ventricle having difficulties to overcome which do not usually exist, will inevitably have a tendency to stretch the vessels both laterally and longitudinally. The result would be the same, were water injected into elastic tubes out of the body. The longitudinal extension will be proportionate to the obstacles opposed to the forward rush of the fluid. The stretching of the arteries in this direction is evidence of the effort which the blood makes to find space.

It is further urged, that a straight artery laid bare is scarcely seen to move, 'but the moment you compress it with the finger, or tie a ligature around it, you perceive it pushed at every pulse.' It is not possible for the eye to detect the pulse in any artery, *unless there be some obstruction to the circulation*. The blood in the human body is estimated at thirty pounds, and one-third is supposed to be arterial. The quantity injected by the left ventricle is probably not an ounce and a half, which cannot make a perceptible difference in the calibre of any artery. To compress it with the finger, or to tie a ligature around it, is either partially or wholly to arrest the course of the blood, so that the vessel is necessarily dilated, and in degree according to the obstacles created. This, however, is stated to be a deception, and the writer attempts to prove it by an experiment on a long vein removed from the body, but which affords no satisfactory results in favour of his opinion.

He remarks that at every additional quantity of water injected, the sensation of dilatation will be communicated to the finger, though the eye can perceive none; and the same will be conveyed to the hand in grasping the leather

tube of a fire-engine, at each stroke of the pump, but the sensation he argues is fallacious, there being no expansion in either case; the phenomenon arising from the tendency of the vein or tube, to '*resume the cylindrical form from the outward pressure of the fluid.*'

Water injected into either must produce dilatation. The fluid which is sent in cannot displace a corresponding quantity without causing this effect, consequently the sensation, so far from leading to erroneous conclusions, is an exact expression of the fact. If the vein be prepared for the experiment and filled with water, it is easy to show that every quantity successively added will occasion dilatation, and to an extent proportionate to the force employed and the quantity injected. Elastic vessels may be filled to various degrees. Their capacity is not uniform and unchangeable. When filled, without the application of any power, save the weight of the fluid, that is one degree of fulness; when an effort is made to send in a further quantity, as this can be received only by displacing a corresponding amount, *during the process of the escape of this amount, the vessels have another degree of fulness.* The one is fulness without unnatural distention—the other is fulness with it. To illustrate the justness of these remarks: suppose the vein in the experiment to be ten inches in length, to contain ten ounces of water, and the object being to inject an additional ounce. In what way does this ounce act? When injected it has the resistance of ten ounces of fluid to overcome. To find space it will press in all directions, and the finger applied to the vein FEELS THE SUCCESSIVE LATERAL DILATATIONS WHICH IT OCCASIONS IN ITS PROGRESS. Did the parietes of the two first

inches of the vein yield to a degree equal to the space required by the additional ounce, no water would be expelled; hence it is evident that in the experiment, the dilatation of the vessel is inevitable, and that the sensation of it communicated to the finger is an accurate indication of the phenomenon.

Immediately preceeding that part of the work, whence the extract is taken, the writer appears to admit the dilatation of arteries. He observes:—

“The HEART *contracts* by *muscular* action and *relaxes* ALTERNATELY: the ARTERIES keep up a CONSTANT *contractile pressure* on their contents, not an alternate contraction and relaxation, but a continued contractile effort *which is overcome by the action of the heart, when there is much blood sent into them they are distended*, and if there be little blood sent into them, as after hæmorrhage, their tendency to contract causes them to close, so as to keep always full, and to preserve a constant stream of the blood even during a temporary relaxation of the heart; and the ARTERIES YIELDING and adapting themselves to the pressure of the heart and recontracting on their contents, whilst the heart is relaxed and filling, is the cause of the EQUABILITY of the STREAM in the VEINS.”

It is here admitted that the arteries exert a continued contractile effort which is overcome by the action of the heart, *and that if much blood is sent into them they are DISTENDED*. The distention of an elastic vessel is the dilatation of it. The one is the consequence of the other, but in ordinary language they imply the same fact.

The next important investigation is the condition of the arterial system immediately preceeding the contraction of the left venticle. All authorities regard it as full of blood; there is, however, great difference of opinion concerning

the manner in which it is influenced by the injection of every fresh quantity. The whole mass of blood is considered to be pushed forward, the arteries at the same time being dilated. The force which is thus expended is imagined to be restored by the recoil of these vessels, so that the power of the heart is transmitted undiminished to every portion of the arterial system. In opposition to this view it is argued, that the small quantities of blood injected by the ventricle are insufficient to produce these results; and, moreover, it is stated, that were such the case, a greater quantity would be discharged by the arteries than is returned to the ventricles. To obviate this difficulty the supporters of the doctrine assert, that the contractions of the arterial system are not *synchronous*, but rapidly *successive* in the intervals of the impulse of the heart. The portion nearest this organ is imagined to be first dilated, which immediately contracting distends the next, and this a third, causing in quick succession a series of dilatations and contractions, the effect of which is to force from the extreme arteries as much blood as is sent into the aorta by any single contraction of the left ventricle. The recoil of one portion is supposed to occur when the next in advance is in a state of relaxation, and that behind is approaching the condition of greatest contraction; so that the blood is easily pushed forward and cannot possibly retrograde. Neither view is founded on ascertained facts, nor does either admit of proof. That the recoil of the arteries urges the blood along its course, with a force equal to that of the heart, is an assumption; nor is it possible to devise any means by which this secondary power can be accurately measured. Neither the experiments of Poiseuille, nor the manner in which blood flows from divided arteries can elucidate the inquiry. The latter fact is alluded to by

physiologists, as furnishing the most satisfactory evidence of the influence of elasticity on the arterial current. Magendie observes in reference to it:

“ Il est cependant très-facile de prouver que ce n'est point ainsi que le sang se meut dans les vaisseaux. Ouvrez une grosse artère sur un animal vivant, le sang s'échappera en formant un jet continu, mais fortement saccadé ; les mêmes phénomènes ont lieu chez l'homme, si les artères sont ouvertes, soit par accident, soit dans une opération de chirurgie ; le cœur ne pouvant pas occasioner un écoulement continu, puisque son action est intermittente, il faut donc que les artères agissent sur le sang : cette action ne peut être que la disposition qu'elles ont à se resserrer et même à s'oblitérer complètement.”*

He contends that were the heart the only propulsive power in operation, the blood would not flow from divided arteries in a continuous stream, marked simply by jerks, corresponding with the contractions of the left ventricle. It would be ejected only contemporaneously with these contractions. It may be shown that the modification in the character of the stream emitted is not explicable on the action of elasticity, and, therefore, the phenomenon cannot with propriety be adduced as illustrative of its agency. The arterial fluid, after the contraction of the left ventricle, independently of the influence of elasticity, is greatly agitated, and has a tendency to flow forward, but its progressive motion is necessarily very limited from the obstacles opposed to its course. Suppose, then, the whole of these obstacles removed, how would the fluid act ? It would not be at rest in the divided artery, but would be urged forward by the rush of

* Journal de Physiologie, tome i, p. 108.

blood from other parts of the arterial system, in the direction of the external opening. Respiration and muscular contractions are constantly modifying the current of blood ; in one situation urging it forward, in another arresting its progress, but in all cases exerting pressure, in consequence of which the blood will escape with accelerated force when an opportunity occurs. A divided artery presents this occasion, and the manner in which the blood is ejected establishes the correctness of the explanation. A much greater quantity is emitted in a given time from the divided artery, than would have flowed through it in the ordinary conditions of circulation.

From this fact two important conclusions may be drawn : first, the tendency of the arterial fluid to flow in this direction, and secondly, that the whole length of the artery and its associated branches are exceedingly distended. The exercise of elasticity in these vessels is at this time impracticable. To contend for its operation is in the highest degree unphilosophical. It is conceived to act in the undisturbed conditions of them, from the consideration of one circumstance, which cannot exist in cases of divided arteries. The arterial system is imagined to be always full of blood, so that the injection of an additional quantity causes distention, of which the arteries relieve themselves by their recoil during the subsequent dilatation of the left ventricle. The recoil takes place, *because the heart does not transmit a continuous stream.* In the case of divided arteries, a continuous current is produced, consequently elasticity cannot be brought into play. How can an artery act upon its contents when it is kept in a state of distension ?

In treating of elasticity, Magendie remarks, “L'élasticité des parois artérielles représente celui du réservoir d'air dans certaines pompes à jeu alternatif, et qui pourtant fournis-

sent le liquide d'une manière continue : et en général, on sait en mécanique que tout mouvement intermittent peut être transformé en mouvement continu, en employant la force qui le produit à comprimer un ressort qui réagit ensuite avec continuité."*

The same kind of evidence is adduced by writers generally in illustration of the influence of the arteries on the motion of the blood. It is, however, anything but satisfactory. A phenomenon occurring in an unnatural state of the vital powers can seldom, if ever afford a correct idea of their action in an undisturbed condition, and certainly the case in question forms no exception. The way in which blood flows from a divided artery throws no light on its motion in the same class of vessels in the ordinary process of circulation. Water in the pipes of a fire-engine is very differently circumstanced from blood in an artery, and the continuous stream which escapes from both is not produced by the operation of similar causes. The prevailing opinion is, that the causes are identical in character, and hence the supposed aptness of the illustration. The water, previous to receiving the stroke of the piston, is at rest, nor would it exhibit any tendency to escape, were the pipes to be punctured in the least dependent situation. The reaction of the reservoir of air, after the stroke of the piston, may be regarded as a second impulse, and acting in a similar manner as the first on the column before it.

In the animal machine the apparatus securing the circulation is not only much more complicated than the one described, but produces correspondingly more diversified effects. The blood-vessels are of three kinds : the arteries, the capillaries, and the veins. The rate of circulation differs

* Magendie, *Journal de Physiologie*, Tome i. p. 110.

materially in each class of vessels. Respiration and all muscular contractions urge or modify the course of the blood. The capillaries, by drawing the blood from the arteries, give it a marked tendency to move forward, and by pouring their contents into the veins, necessarily agitate the current flowing towards the heart. It must also be taken into account, that, independently of the operation of these causes, the previous, not the immediate impulses of the left ventricle, exert an important influence on the motion of the blood. Were the arteries open like the pipes of a fire-engine, the force of each ventricular contraction would be expended on the fluid ejected; but the fluid acted upon still remains to be again agitated or pushed forward; and moreover it is in motion, and would readily escape, without the direct agency of the heart, were any part of the circulating system, but especially the arteries, punctured. An opening in any portion would cause the blood at once to rush out with impetuous force. This would occur were the impulse of the heart interrupted, and not from the elasticity of the vessels, the influence of which is imagined to be aptly illustrated by the reaction of the reservoir of air on the column of water.

The blood escapes with violence from the divided artery, not altogether in consequence of the direct action of the heart, nor of the elastic or contractile properties of the vessel, which is the doctrine taught, *but from the little resistance which the atmosphere offers to the escape of its pent up and active molecules.* The blood which flows first is always propelled to a greater distance than that which subsequently escapes, and the explanation here given accounts for the phenomenon.

The tension of the arterial system is at once relieved by the first portion of the fluid ejected. The continued force with which it flows arises from the action of the heart, and the removal of all resistance in the direction of the external opening. The tendency of the blood to rush in this direction will necessarily convert an intermittent into a continued current, distinguished by jerks corresponding with the contractions of the left ventricle. The water in leather pipes is not thus circumstanced. It is acted upon by one cause only, at any one moment, either the piston or the reaction of the reservoir of air, and it has no tendency to escape, except from the successive action of these causes. Hence it is manifest that the greatest possible disparity exists in the condition of the two fluids, from the striking difference in the complexity of the agents in operation.

The manner in which the blood flows from divided arteries has been regarded, as furnishing conclusive evidence of the laws by which it is regulated in the undisturbed states of the circulation. Magendie indeed draws this inference from the fact, "that in the arteries the blood is not alternately in repose and motion; but that it is moved in a continuous manner with occasional jerking in the trunks and branches, and in a manner uniformly continuous in the ramuscles and their extreme divisions."*

Whether the blood in the interval of the contraction of the left ventricle is comparatively at rest, or urged forward with a force equal to that of the ventricle, is a question requiring elucidation. Experiments have not dissipated the uncertainty in which it is involved, and it is altogether hypothetical to assert that the elasticity of the arteries

* Journal de Physiologie, Tome i. p. 115.

maintains the current in a uniformly progressive movement. Experiments on divided arteries, if they prove anything, show that such is not the case. The blood even under these circumstances is accelerated during the systole of the left ventricle, and is projected with less force during its diastole. Were the two forces equal, such difference ought not to occur; yet it is observed when there are many causes facilitating the flow of the blood during the interval of the ventricular contraction; and it is certainly reasonable to suppose that the difference will be much greater, when the chief of these causes—the removal of all the obstacles to the circulation, does not exist.

It will, perhaps, be evident from these strictures that the exercise of elasticity is not proved by the character of the stream ejected from divided arteries. It is not, however, to be understood that I deny entirely the influence of this property on the motion of blood. Its existence was as fully appreciated by the earlier as by modern physiologists, nor have the elaborate researches of the latter added anything of value to our previous knowledge of this interesting but abstruse question. The arterial fluid is admitted to become slower as it proceeds in its course; and can it be doubted that the circumstances which cause this effect diminish the pressure or force with which it circulates? Friction, and the blood passing into vessels, the conjoint area of which augments at every step, will enfeeble its motion. The fact of its flowing from divided arteries of the toes with much less violence than from those of the mammæ, throws, indeed, more light on this difficult subject than any other phenomenon.

The account which is given of capillary circulation by Poiseuille and Magendie, does not strengthen the opinion that the pressure of the heart is transmitted directly to the

venous system. It requires some effort of the mind to conceive the globules as carrying forward the momentum impressed upon them by the left ventricle, and as being in fact parts of one uninterrupted chain extending from the left to the right side of the heart, without the aid of which circulation would be arrested in the veins. This is the doctrine taught, and yet these globules, so important as links of continuous motion, are described as sometimes being almost at rest—carried in a retrograde direction, or as traversing singly the capillaries, or two or three abreast, often with very little progression.

Were the arterial and venous systems united by obvious currents of blood, the influence of the one might be imagined to be transmitted to the other, but when the connexion is considered to be maintained only by globules, frequently pursuing their course alone, or moving in a devious direction, the doctrine, that large masses of venous blood are propelled by them, is not without serious difficulties. In what manner are they affected? Do the small streamlets first formed by the capillaries act upon the columns before them, and these upon others still larger, until at length the contents of the *venæ cavæ* receive the impulse of the heart? Globules circulating in the way described cannot possibly propel masses of blood.

Leaving this subject for the present, I pass to make a few further remarks on the condition of the arterial system in the systole and diastole of the ventricle.

The quantity of blood injected into the aorta at every contraction of the left ventricle is computed at about an ounce-and-a-half. The energy with which it is propelled is expended against the arch of the aorta, producing a vibratory motion, and upon the mass of blood before it. The arterial distension produced by the heart is modified by

the recoil of the arteries during the diastole of the ventricle. The difference in the motion of the arterial fluid, or of the forces employed in the systole and diastole of this cavity, is a problem which physiologists have not been able to solve. The contraction of the ventricle and the subsequent recoil of the arteries urge forward a quantity exactly proportionate to what is received, in the same time, by the right auricle. Were the former alone to displace a quantity equal to what is sent in, the additional action of the arteries would disturb the relation between the two cavities, by transmitting to the veins more blood than could possibly be removed by the right side of the heart. The quantity injected by the ventricle displaces a quantity less than that sent in, by as much as is required to distend the arterial system.

The difference in the rate at which blood moves in the arteries during the systole and diastole of the heart is perhaps a question which will ever remain undetermined. The study of the phenomena of circulation certainly leads me to the conclusion, that the arterial fluid is urged forward slowly and feebly in the diastole of the left ventricle. The condition of the aorta, after the contraction of this cavity, does not appear favourable for the exercise of elasticity. The blood injected, in acting on the column before it, will probably leave a space immediately at the roots of this vessel only partially occupied, so that in the diastole of the ventricle a small quantity of blood will have a retrograde motion, and, according as this relieves the arterial distension, the agency of elasticity will be proportionately diminished.

The supposition which is here advanced respecting the condition of the arch of the aorta, during the diastole of the left ventricle, receives, from the researches and reasoning of others, confirmation previously unknown to me. The fact

is not, however, employed to show its effects on the exercise of the elastic properties of the arteries. If it exists it is impossible to deny that the tension of the whole of the arterial system is modified by it. Burdach remarks :

“ Si, en outre, l'onde de sang est poussée jusqu'à une certaine distance dans l'artère, par l'effet de la systole, mais reflue en suite (§ 714, 6°) vers les valvules sigmoïdes, qui se sont cependant fermées d'elles-mêmes (§ 708 3°), ce phénomène suppose également un espace qui ne soit point occupé en entier par du sang, et comme, pendant la diastole du cœur, le sang continue de couler du tronc artériel dans les branches (§ 714, II), mais que ce tronc n'est point assez flexible pour pouvoir se contracter dans la même proportion, nous sommes portés à admettre également ici un vide, ou un espace contenant de l'air. Fontana oppose à cette opinion une expérience faite par lui, et d'après laquelle l'aorte, quand il la piquait auprès des valvules sigmoïdes, donnait du sang pendant la diastole du cœur, en moins grande quantité seulement que durant la systole. Mais on ne saurait rien conclure de là contre nous ; car nous admettons seulement qu'une certaine étendue de l'aorte n'est point remplie en totalité, nous ne prétendons pas qu'elle soit absolument vide, et quand bien même ce dernier cas aurait lieu, le sang n'en coulerait pas moins par la plaie. Spallanzani (1) a vu, sur des Salamandres, que le bulbe de l'aorte contenait peu de sang pendant la diastole des ventricules, mais qu'il n'en renfermait pas du tout quand la circulation était affaiblie, de manière qu'alors il avait une couleur pâle, et ne laissait échapper aucun liquide quand on l'ouvrait.”*

* Traité de Physiologie, par. C. F. Burdach, Traduit, by A. J. L. Jourdan. Vol. vi. p. 283.

Modifications are continually occurring in the distribution of the blood requiring corresponding changes in the capacity of the arteries. At one period these are greatly distended, at another the vital energies are so exhausted that the pulse is scarcely felt. How immense is the difference in the quantity of blood circulating at these times ! The arterial system is not only liable to be thus influenced by disease, but by the every-day pursuits of life. In one part, circulation is impeded, in another it is accelerated, the blood being urged forward with a strong and irregular force. These varying conditions demand commensurate alterations in the capacity of the arteries, and which can be secured only by their elastic and contractile properties.

The experiments of Magendie and Poiseuille have been shown, in the preceding pages, not to warrant the conclusions deduced from them. The former physiologist adverts to the practical importance of the views founded upon the supposed fact, that the blood is moving with an equal momentum throughout the animal system. He remarks :

“ Cette égalité de pression dans l'universalité du système artériel est un fait fort important relativement à la pratique. Si vous voulez diminuer la masse des liquides, il importe peu que vous ouvriez telle artère plutôt que telle autre ; car l'équilibre est simultanément rétabli dans les conduits vasculaires, et les parois de chaque tuyau se trouvent soulagées d'une pression partout uniforme. Il est des médecins qui attachent une grave importance au choix de l'artère qu'il convient de saigner. Aussi la temporale est-elle généralement préférée dans les cas d'affection cérébrale : ce sont là des prétentions scolastiques qui n'ont aucune espèce de fondement, que repousse une saine

théorie. La vérité ici comme dans beaucoup d'autres circonstances, est plus simple que ce que l'imagination avait enfanté ! A quoi bon se fatiguer la mémoire de tous ces préceptes erronés, consignés dans tous les livres, sur le vaisseau qu'il convient de choisir dans telle ou telle maladie ? Le seul fait d'égalité de pression simplifie singulièrement la question."*

The experiments supposed to demonstrate the equality of arterial pressure have been shown to be liable to serious objections, and, consequently, the application of this principle to medical science may be fraught with much mischief. Experience has repeatedly proved the value of the practice which the writer ridicules, and it would be highly injudicious, on many occasions, not to make the selection of the artery an important consideration. Indiscriminate local bleeding might effect the desired end ; but this would not prove the justness of his views. The relief might arise from the treatment subduing irritation—the cause of the urgent symptoms ; or the action of the heart might be quieted by it ; or, indeed, much of the good attributed to the measure might be owing to the conjoint employment of means, whose beneficial agency might be entirely overlooked in the calculation.

Several other important experiments of Magendie and Poiseuille, bearing especially on the tension of the arteries and veins, will subsequently be analysed, and they will be shown not to establish an equality of pressure throughout the arterial system. Previous to the adoption of the doctrine, that the selection of the artery is quite a matter of indifference, the evidence on which it rests should be free from all doubt and speculation. The experience of ages

* Leçons sur les Phénomènes Physiques de la vie, Tome iii. p. 42-3.

is opposed to it ; and it is at variance with the sentiments of all antecedent physiologists.

After the preceding investigations, the pulse naturally comes under consideration. There is now little difference of opinion respecting the cause of it. The view which Parry entertained is generally admitted, and its accuracy appears to be placed beyond all question by elaborate and carefully conducted experiments. There are points, however, connected with the subject on which writers are by no means agreed. It is contended by one class that, though the pulse is produced by the impulse of the left ventricle, the contractions of the arteries in different parts of the body are not synchronous, but rapidly successive. The earlier physiologists distinctly state that the contractions of the arteries are synchronous with the systole of the heart, and my own researches are decidedly in favour of this conclusion. The following extract gives some of the authorities who think otherwise :—

“ Weitbrecht, Liscovius, and E. H. Weber * have shown however, that this is not the case. The pulsation of the arteries near the heart is synchronous with the contraction of the ventricle. But at a greater distance from the heart, the arterial pulse ceases to be perfectly synchronous with the heart's impulse, the interval varying, according to Weber, from one-sixth to one-seventh of a second. Thus, the pulse of the radial artery even is somewhat later than that of the common carotid. The pulse of the facial, at about the same distance from the heart, is isochronous with the pulse in the axillary artery ; while the pulse is felt somewhat later in the metatarsal artery on the dorsum of the foot, than in the facial artery and common carotid—

* In the Treatise, *De Pulsu non in omnibus Arteriis plane synchronico.*

Weber* has explained the cause of this difference. If the blood circulated in perfectly solid tubes, whose walls admitted of no extension, the impulse of the blood, driven by the ventricle into the arteries, would be communicated even to the end of the column of blood, with the same rapidity with which sound is propagated through this fluid,—much quicker, namely, than in atmospheric air; the pressure of the blood would be transmitted to the finest extremities of the arteries, with no perceptible loss of time. But, in consequence of the arteries admitting of some extension, particularly in length, the impulse given to the blood by the heart distends first merely the arteries nearest to the heart. These, by their elasticity, again contract, and thus cause the distension of the next portion of the arterial system, which also, in its turn, by contracting, forces the blood into the next portions, and so on; so that a certain interval of time, although a very short one, elapses before this undulation, resulting from successive compression of the blood, and the dilatation and contraction of the arteries, reaches the most distant branches of the arterial system.”†

The same view is also adopted by Carson in his *Inquiry into the motion of the blood*, and is supported with his usual ingenuity.

Haller remarks, “*Sed etiam in homine, si manum dextram cordis sedi opposueris manum sinistram arteriæ temporali, labiali, radiali, popliteæ adplicueris, manifesto percipies, eodem omnino tempore et cordis recurvatum apicem costas ferire, et sanguinem in omnibus arteriis, quas nominavi, pulsum efficere. Experimentum sæpe feci, et*

* Annotat. Anatom.

† Muller's *Elements of Physiology*, p. 200, 201. Translated by William Baly, M.D.

in me, et in vivis animalibus; fecit Harveius; † fecerunt primi eircuitus sanguinis statores; § fecerunt nuperi Cl. viri; || fecit in equo Cl. Bourgelat." ¶

This experiment I have repeatedly performed with the greatest possible care, and under circumstances favourable for the attainment of truth, and always with the same result. It is stated that 'the impulse given to the blood by the heart distends first merely the arteries nearest to the heart. Whether such impulse be directed against a column of blood in arteries possessing elasticity, or altogether destitute of it, one effect must take place,—*the forward movement of the fluid acted upon*. Between arteries and inorganic tubes there is a difference which would simply modify the effect. In the former the blood injected by the ventricle would not remove an equal quantity from the extreme arteries, but only so much as exceeds the distension of the arterial system. In the latter an equal quantity of fluid would necessarily be discharged. The pulse unquestionably depends on this forward rush of the blood, and it follows as an inevitable consequence, that the impulse must be transmitted instantaneously to all parts of the arterial system. To assert that the blood propelled distends only the arteries nearest to the heart, is equivalent to saying, that it does not act on the column in advance. If it does, the impulse of the ventricle will be felt at the same instant throughout the body.

In opposition to the view which is here advocated, Dr. Carson remarks, "upon the supposition of a synchronous contraction of all parts of the arterial system, the quantity of blood discharged in consequence of that contraction,

† Exercit. i c. 3.

§ J. Walæus, Epist. cit. p. 406, ed. 1.

|| Josephus Duverney de l'ouïe, p 206, de pulsu in aure locutus. Thomas Schwenke, 1. c. p. 82. ¶ Hippia trique, Tome ii. p. 346.

from the termination of the arteries, must greatly exceed that which would be returned to the ventricles, even if there were no valves to oppose it; the quantity which would be discharged from the former, would be to that returned to the latter, as the square of the diameter of all the capillary arteries of the aortic system, to that of the diameter of the aorta alone.”*

The simultaneous contraction of the arteries will not produce the result which is here stated,—in fact, it may be shown to be an impossibility. It is admitted that the arteries are full previously to the injection of every fresh quantity into the aorta, and however greatly opinion may be divided concerning the functions of the arteries, on one point there can be no difference, viz., THAT THEY WILL BE DILATED, THOUGH NOT TO ANY PERCEPTIBLE DEGREE, BY EVERY ADDITION TO THE MOVING COLUMN. The arteries are not rigid unyielding tubes, so that a dilatation to some extent must take place. Hence the quantity of blood injected by the left ventricle produces two effects,—*the forward rush of the column, and the dilatation of the arterial system.* Nor can the discharge of blood from the extreme arteries, on the supposition of their simultaneous contraction, possibly exceed that which would be returned to the ventricles in the same time.

During the dilatation and contraction of the arteries, the quantity discharged is precisely equivalent to that which is transmitted in the same time to the cavities of the right side of the heart. There are two periods to be taken into account, the one occupied by the impulse of the left ventricle, and the other by the contraction or

* The expression should be “the sum of the squares of all the diameters of the capillary arteries of the aortic system.”—An inquiry into the causes of respiration; of the motion of the blood, &c. By James Carson, M.D., second edition, p. 102.

subsidence of the arteries. Writers leave out of consideration the second period.

This view explains satisfactorily the way in which the balance of circulation is maintained between the arteries and veins, without the necessity, which is implied in the doctrine of Weitbrecht, Liscovius, Weber, and Carson, for the successive contractions of the arteries.

In connexion with the analysis of the functions of the arteries, their usually empty state after death is a phenomenon which is attempted to be solved on very different principles. By one it is contended that the elasticity of the arteries, at the moment of death, expels the blood contained in them. By another the effect is ascribed to the tonic contractions of the arteries. By a third the resilience of the lungs and of the coats of the arteries is regarded as the efficient cause; and by a fourth the vacuity of the arterial system is imagined to arise from the attraction of the capillaries. It is not my intention, on this occasion, to enter into a lengthened analysis of the phenomenon, as it will be subsequently noticed in investigating the functions of the capillaries. In the researches on this subject, no attention appears to have been given to the gradual changes which generally take place for hours previous to death.

During the period of dying, the action of the heart gradually becomes feeble and mostly frequent, until at length it ceases to be detected in the slender streamlet flowing along the arteries, though it may still be discovered on applying the ear to the chest. Whether the period be long or short, the venous system is slowly acquiring a preponderance over the arterial, not, indeed, from any of the causes enumerated, *but from the exercise of the ordinary powers of circulation*. At the moment of death the arterial system contains scarcely any blood.

Carson endeavours to show, that the difference in the distribution of the blood after death, from what exists during life, "arises chiefly from the elastic power of the lungs." To establish this, he performs several experiments. In one case, "the belly of a rabbit was freely opened from the scrobiculus cordis nearly to the pelvis," and then two large incisions were made through the muscular part of the diaphragm. In the other, a sharp instrument was thrust between the vertebræ of the neck. Both animals *instantly* died. In the former, he says, the vessels of the intestines and stomach were full of blood, and "the flesh was reddish, and when cut into, bled." In the latter, there was "scarcely the vestige of a blood vessel to be observed on the surface of the intestines or stomach. The flesh was white, and when cut into appeared dry." The vascular condition of the viscera and of the flesh, is what might have been anticipated. But how the whole of the blood should instantaneously be withdrawn from the body of the rabbit into the trunks of the veins and chest, is to me inexplicable. If this is to be referred to the elasticity of the lungs, the influence which it exerts is far more powerful after death than during life.

Were the vacuity of the arteries occasioned by any of the causes assigned by physiologists, there ought to be no difference in the results. Why should not the resilience of the lungs, the elasticity or tonic contractility of the arteries, operate equally in all instances? If any difference existed it should be in favour of their greater activity when death is sudden, whether arising from hanging, drowning, suffocation from irrespirable gases, or any other circumstance, the natural properties of the lungs and of the arteries not having been wasted by participating in any previous structural changes, which always enfeeble and

exhaust the vital energies. When these properties have been the least impaired, such as continue in action for a short time after dissolution ought to exercise greater influence than in cases preceded by the decay of the bodily powers.

When death is the consequence of protracted disease, the circulating fluid is greatly diminished in amount, in many instances, at least, to the extent of one-half. There is no permanent decrease in the capacity of the veins corresponding to this change. It may therefore be stated, that there is a lessened quantity of blood, and an unaltered capacity of the veins. In cases of sudden death there is no diminution in the quantity of the blood, therefore it would scarcely be expected, whatever causes may be supposed to be in operation, that this would have the same facility of escape from the arteries into the veins, as when the animal system has been subject to the process of gradual exhaustion. Both classes of vessels are full, to the ordinary distension of health, which is very different from their condition after the long continuance of organic mischief.

The striking differences in the state of the circulating system, in the various cases of death, may be thus briefly stated :

1. When death is the consequence of disease, the arterial system is usually found empty, and almost the whole of this effect occurs antecedently to the extinction of life.

2. The very slender streamlet which may continue in motion, will certainly not be urged forward by the elasticity or any other property of the arteries. These are not found after death contracted or cord-like, and nothing less than the exercise of a power producing this condition, would act on the attenuated and lingering current.

3. In case of death from hanging, drowning, or any unnatural cause, the arterial system, is seldom, if ever, found in a completely empty state.

4. Previous to death from any of these circumstances, the arterial and venous systems are full to the ordinary distension of health, and therefore it is scarcely reasonable to anticipate that the venous should be able to receive readily the entire contents of the arterial.

5. There is also a remarkable difference in the two cases of death, with respect to another class of vessels, viz., the capillaries, which is worthy of consideration. Their action will be admitted to continue after dissolution; but when this has been preceded by long suffering, they have clearly, in common with every other part of the body, nearly ceased to exercise their functions. When however, it occurs suddenly, these being unimpaired by disease, their surviving agency will tend to draw the blood from the arteries and transmit it into the veins; but the latter not being able to receive the whole of the arterial fluid,—undiminished in quantity,—the arteries will seldom be found in a state of vacuity as when death is the result of tedious decay.

6. In many cases preceding death, the breathing is sometimes so exceedingly soft that it is difficult to perceive it; consequently the resilience of the lungs, after the last expiration, can scarcely, at this time, be supposed to produce any important change in the circulating system.

BOOK III.

THE INFLUENCE OF THE HEART ON THE MOTION OF THE BLOOD.

According to the views of one class of physiologists the action of the heart is transmitted through the sanguiferous system, and is the sole cause of circulation. According to the reasoning of others it terminates at the capillaries, and other agencies are imagined to be brought into operation by which the blood is enabled to perform its extended revolution. Others, again, regard the heart as urging the blood beyond the capillaries, but ascribe the motion of it in the great veins to the suction power of the right auricle, or to alterations in the capacity of the lungs. Amidst such discrepancies the mind is confused in its attempt to arrive at general principles.

An equal amount of talent has been employed in the support and illustration of these various doctrines, hence the difficulty of the undertaking, which proposes to point out the degree of fallacy and truth involved in each, nor will it be materially lessened by the institution of new, or by the repetition of old experiments. Several of these are objectionable, the mode in which they were conducted being

incapable of furnishing correct data; and further, they have not been studied with sufficient attention in respect of the changes produced by them in the condition and relation of important organs, so that the results to which they have led, have been false, partial, or of questionable utility.

The attainment of just and enlarged views on this subject is obstructed by causes which are by no means easy to surmount. To measure the force of any propulsive power carrying on the functions of life, under ordinary circumstances, and in co-operation with other agents, the individual and conjoint influence of which is liable to be aggravated or destroyed by every direct interference, is a task which can scarcely be entered upon with the hope of perfect success. The calculations, however elaborate, can only be an approximation to the truth. Let the mind for a moment consider the object in view, viz., to determine the impulse of the heart on the motion of the blood; and weigh, also, the intricacies involving every step of the inquiry. The nature of the soil proposed to be turned up ought to be well understood.

An important distinction has been lost sight of in the prosecution of this matter. The force of the heart, imagined to be established by experiment, has been regarded as identical with that in operation in the natural or undisturbed conditions of the animal system. These, however, are two questions very dissimilar, and by no means presenting the same difficulties. If it be admitted that the circulation is maintained by the heart,—the elasticity of the vessels,—the contractility of the capillaries,—the changes in the capacity of the chest,—or by one or more of these powers,—it may be shown, that, in endeavouring to ascertain by means of experiments, which mutilate the parts examined, the agency of any one, but especially that

of the heart, the influence of the whole, and their relations to each other, will be seriously deranged.

1. One source of fallacy may be the following :—In the operation of laying bare important blood-vessels, the mental suffering and agitations of the animal are invariably great, and, therefore, whatever result is obtained under such circumstances is comparatively of little value. The amount of the impulse usually co-operating in the circulation of the blood remains to be discovered, and will long be one of the mysteries of nature.

2. If, by our interference, the character of the current is materially changed. The powers by which it is urged along may be invigorated, enfeebled, or annihilated ; so that experiments producing this effect cannot possibly lead to any accurate conclusions. It will subsequently be shown that physiologists, in the prosecution of this inquiry, have calculated none of these difficulties, but have pursued their course, as if they had to deal with dead or inorganic substances.

3. The mind should not lose sight of the important fact that all vital actions are regulated by laws, delicate and finely adjusted ; and that to touch rudely the matter over which they preside, is to disturb the sympathetic and harmonious concord uniting into one beautiful whole their complicated operations. Were the physiologist, in his researches, to feel the full force of this truth, reflection would often take the place of action ;—boldness and decision would be converted into hesitation ; and immature theories would frequently assume the modest form of hypotheses, to the advantage of science. In none of the numerous experiments performed to determine the propulsive power of the heart, were the consequent disturbed conditions of the capillaries, and

of the arterial system generally, even suspected, much less taken into account. The necessity of appreciating these is too evident to require illustration.

All the vessels of the body may be divided into two great classes : those whose office it is simply to convey fluid from one part of the system to another ; and those in which the essential operations of life are carried on. The contents of the arteries, belonging to the first class, are indisputably urged forward by the heart ; but it has yet to be demonstrated that the motion of blood in capillary vessels is to be traced entirely to the same cause. The existence of a *vis-a-tergo* is no proof ; nor do the researches of physiologists clearly establish the fact. The circulation peculiar to the liver cannot with propriety be ascribed to the impulse of the left ventricle. The blood, before it arrives at that important viscus, has passed through arterial and venous capillaries ; and it is again distributed to, and collected from such vessels, previously to entering the *vena cava*. Can the heart be regarded as the efficient agent of circulation in four series of capillaries, one arterial and three venous ? The effect would not only require an extraordinary degree of motive power, *but manifest distension of the multitude of vessels extending from the left ventricle to the hepatic veins, without which no such central influence could be transmitted.*

The consideration of the varying conditions of the sanguiferous system, and the phenomena of capillary circulation, as exhibited in the field of the microscope, renders it difficult to imagine such extensive influence. The abdominal vessels, and especially those of the liver, are often greatly congested. The accumulation of blood is evident from local and constitutional symptoms, which can scarcely

be misinterpreted. A condition of this kind must be a barrier to the transmission of any central power. The obstacles opposed to its conveyance are clearly great. It must also be remembered that while, on the one hand, the mass of blood to be moved is augmented, on the other, the force by which it is supposed to be impelled is enfeebled, hence it is not easy to conceive that the contents of the last series of capillaries are, at this time, urged forward by every contraction of the heart.

The motion of the blood in capillaries, which has frequently been minutely observed and accurately described, does not afford evidence in favour of such doctrine. The globules, which are the only connecting links between the columns of blood in the arteries and veins, are not characterised by a regular progressive march. At one moment they retrograde ;—at another they appear to revolve upon their axis, or move forward only by fits. Does it seem probable that they are the bearers of the influence of the heart to the masses of fluid in the great veins, propelling them in their course to the right auricle ? Were this the occasion for adducing the various facts bearing on this subject, the way in which circulation is carried on in the fœtus, and especially in cases of monstrosity in which the heart is wanting, would be found to give no support to such an opinion.

The next step is to analyze important experiments, which, from their high authority and apparent conclusiveness, are imagined to establish the doctrine which is here combated. Those of Magendie have the first claim to attention. His acknowledged ingenuity and enterprise justly entitle him to this distinction. The following experiment is universally admitted to demonstrate the influence of the heart on venous circulation. It is alluded to by all

writers in corroboration of this fact ; and no one has presumed to invalidate its force by the discovery of any defect in its conception or execution. He observes :—

“ After having passed a ligature round the thigh of a dog, as I now described, that is, without including the crural artery or vein, apply a ligature separately upon the vein near the groin, and then make a slight opening in this vessel. The blood will immediately escape, forming a considerable jet. Then press the artery between the fingers to prevent the arterial blood from reaching the member. The jet of venous blood will not stop on this account ; it will continue some instants ; but it will become less and less, and the flowing will at last stop, though the whole length of the vein is full. If the artery be examined during the production of these phenomena, it will be seen to contract by degrees, and will become completely empty. The blood of the vein then stops ; and at this period of the experiment, if you cease to compress the artery, the blood injected by the heart will enter, and as soon as it has arrived at the last divisions, it will begin to flow again at the opening of the vein, and by little and little the jet will be established as before.”*

The same experiment, very recently performed by him, is also introduced here, in consequence of suggesting some important remarks. “ When I compressed,” he says, “ the trunk of the crural artery, the stream from that vein diminished, as though it would be arrested altogether. On the contrary, when the artery was freed from pressure, and the passage of blood through the smaller vessels was unimpeded, the jet from the vein returned with its original force. I repeated this, moreover, several times with the same effect,

* An Elementary Compendium of Physiology, Trans. by E. Milligan, M.D., 4th edit. p. 422.

—at least with the effect of diminishing the flow of blood from the vein, for I do not know why I could not succeed in arresting it altogether. 'The experiment shall be repeated more perfectly. What you have seen, however, is sufficient to establish the fact that the influence of the heart is extended to the venous circulation.'*

The experiment in neither case affords satisfactory evidence in favour of the doctrine insisted upon.

Were the blood in the vein circulating from the direct impulse of the heart alone, it would clearly be urged forward by every contraction of the left ventricle, and would, necessarily, be almost at once arrested on the interruption of the impulse: but this appears not to have been the case. On making a slight opening in the vein, he observes: "The blood will immediately escape, forming a considerable jet. Then press the artery between the fingers, to prevent the arterial blood from reaching the member; the jet of venous blood will not stop on this account, it will continue some instants; but it will become less and less, and the flowing will at last stop, though the whole length of the vein is full." It is here distinctly stated that the flow of venous blood was not instantaneously arrested, on interrupting the action of the heart; but continued for some time. The experiment is to prove that the jet is the effect of the direct impulse of the heart, the contrary, however, is established by it. The gradual diminution of the jet, until at length it stops, is a result which might have been anticipated from the altered conditions of the circulation. The blood flows from the punctured vein, as long as the capillaries receive arterial fluid, and ceases only when this fails to be supplied. That these vessels

* The Lancet, vol. I. 1834. p. 376.

are destitute of blood, is evident from the vacuity of the crural artery.

The fact on which Magendie dwells as peculiarly striking and unexpected is, *that at last the blood ceases to escape, though the whole length of the vein is full.* There is nothing remarkable in this circumstance, nor does it indeed prove, as he imagines, that the column of venous blood is at rest from the action of the heart being interrupted. Did the fact establish the conclusion which he draws, it ought unquestionably to be perceived immediately on arresting the motion of blood in the artery. The venous column is not at rest until the whole length of the artery is empty. This condition, though inexplicable on the views of Magendie, may at once be satisfactorily accounted for. As long as the capillaries connecting the artery and vein are furnished with blood, they are enabled to propel their contents into the latter vessel, but the power they exercise will clearly become feeble and inefficient in proportion to the diminution of the supply, until at length it entirely ceases.

The further remarks of Magendie confirm this explanation. "At this period of the experiment," he says, "if you cease to compress the artery, the blood injected by the heart will enter, and as soon as it has arrived at the last divisions, will begin to flow again at the opening of the vein, *and by little and little the jet will be established as before.*" Hence the blood does not flow from the vein until it has reached the last divisions, that is, until the capillaries are abundantly furnished, and then the jet is established by little and little, and not at once as would be the case, were the capillaries already possessed of blood. Were they in this condition, the impulse of the heart ought to be transmitted immediately to the venous column, on the removal of pressure from the artery.

As this physiologist is partial to illustrations of vital phenomena derived from physieal science, I will offer one for his consideration. Suppose a series of metallie tubes filled with water. Every additional quantity injected would clearly displace an equal amount from the extremities. The jet in this case would not be gradually established, but at once ; nor would it become weak by little and little, on the cessation of the impelling power. In his experiment, it is not necessary to take into account the modifying influence of the vessels through which the blood flows. The action of the heart alone solicits attention, and, therefore, this illustration is not very remote in the justness of its application.

The time required, in the experiment of Magendie, for the re-establishment of the jet shews, as just remarked, that the mass of vessels, from the distended vein to the point of the compressed artery, is empty ; and though the blood subsequently flows, it affords no evidence of the direct action of the heart. The phenomenon exists for some instants after the influence of this organ is arrested, and is not perceived for some time after the removal of all pressure from the artery. In a recent attempt by the same physiologist, the experiment was not successful. He acknowledges, that in compressing the artery, the escape of blood from the vein was only diminished. In explanation of the failure, the dog is stated to have been weak from the previous injection of water into the veins. Is it therefore to be inferred, that when the powers of life are enfeebled, blood moves in the veins without the assistance of the heart, and that when vigorous, it circulates from the impulse imparted by this organ ? If it is a law that this fluid is urged forward by the heart, the effect ought to be as obvious in one state of the animal system as in another.

Indeed the influence of this organ seems most required when the vital energies are weak ; for at this time the blood is less capable of stimulating the mass of vessels in which it circulates. Such is the experiment to prove that the motion of the venous blood depends on the action of the heart.

Bichat regards the capillaries as the principal cause of its motion. No experiment which seriously deranges the functions of the circulatory system can possibly determine the question. In the procedure of Magendie, how are the capillaries circumstanced ? In pressing upon the artery, until the stream ceases to flow from the punctured vein, they are clearly deprived of blood ; and therefore, with what justice can it be asserted that venous circulation is unaffected by them ? Whether the motion of blood in these vessels is derived from the heart or not, *it will unquestionably be modified by every interruption or disturbance of the arterial current.*

A little further consideration of the experiment of Magendie will show that it is quite incapable of affording any accurate results. He proposes to ascertain the cause of the motion of blood in the veins ; but, in attempting to accomplish this, he completely destroys the ordinary conditions of the arteries, capillaries and veins. The natural relations which these vessels bear to each other no longer exist. Previously to pressure being applied to the artery, the blood *flowing* from the punctured vein, is a circumstance alone quite sufficient to invalidate the conclusions deduced by him. Its mode of circulation in the undisturbed state of the animal system is very different. In the one case, there are obstacles to be overcome, or its motion is constantly liable to interruptions from causes influencing the columns of blood before it ; in the other, there are no impediments, but indeed its flow is greatly accelerated

by the removal of the ordinary causes of retardation. *To facilitate its escape in this manner, is to modify the current both in the capillaries and arteries, as if the pressure acting a-tergo was increased.*

The dilatation of the cavities of the heart, in all probability, accelerates the flow of blood towards them. It has a ready means of escape from the tension by which it is influenced, at the successive periods in which the partial vacuum occurs. The difference between the resistance of the atmosphere and the contents of the vessels, is manifestly great. Its escape from a divided vein is accelerated and violent, not in consequence of the exercise of an extraordinary force *a-tergo*, but from the removal of all obstacles to its flow. The atmosphere, compared with the ordinary resistances to the circulation may be viewed in the light of a vacuum, towards which the blood will direct its course with impetuosity. When it is interrupted in the artery, it still flows with undiminished freedom from the vein. This cannot be ascribed to the direct impulse of the heart, for this is arrested. The capillaries, indeed, convey forward the fluid as long as they are supplied, and it continues to escape from the vein until the whole length of the artery is empty. Hence it is evident that this experiment affords little, if any, precise information on the cause of venous circulation, or its degree of dependence on the heart. If it prove any thing, it establishes the independent action of the capillaries.

The objections which are here urged against the experiment of Magendie, apply with equal force to his reasoning on the effects of venesection and syneope, which are referred to as evidence of the influence of the heart on the circulation. The former, when carried to a serious extent, enfeebles, and the latter often arrests the action of this

organ. They both disturb the functions of the capillaries, and almost as much as pressure on the artery. Syncope is not an instantaneous effect. However induced it is preceded by a feeling difficult to define. Previously to any symptom of uneasiness, the countenance changes colour, and occasional deep inspirations occur. The heart struggles to carry on the circulation, until at length its action virtually ceases. From the first moment of uneasiness, to the cessation of its contractions, the blood is slowly leaving the arterial, and accumulating in the venous system ; and in all probability the larger arteries are nearly empty when syncope takes place.

One unquestionable office of the capillaries is to convey forward the blood transmitted to them ; and as long as the supply is abundant and regular, the exercise of this function will necessarily influence the circulation in the veins ; when, however, it ceases to be furnished, as in syncope, the action of the capillaries is interrupted, and the tension of the sanguiferous system is so far diminished, that the vital fluid has no tendency to escape from the punctured vein. Nor is the jet immediately re-established on the return of the contractions of the heart. The blood may trickle from the external opening, but it is some time before it is projected with energy. Thus the capillaries, and the great veins, are in a condition similar to that produced by pressure on the artery in the experiment of Magendie ; and the same explanation will apply to both cases.

The dependence of capillary circulation on the impulse of the heart has also been inferred from the facility with which injections are made to pass from arteries into veins. The force employed is stated to be considerably less than that of the left ventricle. There is, however, no just estimate

of this force. The calculations of Borelli, Keill, and Hales; and their followers, in the same inquiry, Bernouilli, Sauvages, Jurin, Bryan, Robinson, and others, exhibit an extraordinary discrepancy. Nor do the calculations of modern physiologists by any means harmonize. Between those of Arnott, founded on the researches of Hales, and the recent calculations of Poiscuille, there is a difference of above fifty-five pounds.

It has already been remarked that the contents of the arteries are urged forward chiefly by three forces; the direct contraction of the left ventricle, its previous impulses, and, to a slight degree, the elasticity of the vessels themselves. But as these are not in action at the same moment, the energy with which it is propelled necessarily varies with the causes in operation. The alternation in the play of these forces is well calculated to allow the capillaries to exert their contractile powers. Were they in a state of extreme tension, they would be incapable of propelling their contents; and therefore would be altogether passive in the process of circulation. But what is the condition of these vessels, when water is injected, to ascertain the facility with which fluids are transmitted through them? From the point of the artery, to which the apparatus is applied, to the corresponding vein, the *capillaries intermediate are in a state of excessive tension*, so that the fluid will traverse them with almost the same facility, that it would inorganic tubes; a condition incompatible with the exercise of their functions; and consequently the experiment cannot be adduced as illustrative of the natural influence of the heart. It indeed admits of one positive inference only, viz. the direct communication between arteries and veins. The powers which facilitate or retard the motion of blood in the animal system,

cease on the extinction of life ; and, therefore, the injection of fluids through a series of vessels destitute of vitality, affords no estimate whatever of the impulse of the left ventricle, nor, indeed, any comparative approximation to a correct calculation.

According to Arnott, "The arterial tension of four pounds to a square inch, marked by its supporting, in a tube connected with the arterics, a column of blood eight feet high, is produced by the action of the heart ; but as the heart, while injecting the blood against this resistance, has, moreover, to overcome the inertia, both of the quantity injected, and of the mass in the great artery, first moved by the injection ; as also the elasticity of the vessel yielding to momentary increase of pressure ; the heart must act with a force of about six pounds on the inch. Now, as the left ventricle of the human heart, when distended, has about ten square inches of internal surface, the whole force exerted by it may be about sixty pounds."* This calculation rests entirely on the experiments of Hales. The area of the left ventricle, and the height to which blood would rise in a tube fixed to the carotid or any large artery, being first ascertained, and then multiplying the square inches of the area, by the height of the column, the pressure of the ventricle is imagined to be determined. An examination of the impulse of the heart certainly does not convey the idea of any extraordinary force being exerted. It would never be supposed that this was equal to sixty pounds.

The opinions of Arnott demand respectful consideration. Distinguished as they are by great acuteness, and an intimate knowledge of physical science, it may appear

* Elements of Physies or Natural Philosophy. By Neill Arnott, M.D.

presumptuous to question their accuracy. If the subject were one to which the principles of physical science could be justly applied, it would be bold in the extreme to hazard an objection. But when the principles of an exact science are employed to elucidate the mysteries of one which embarrasses by the variety and intricacy of the operations crowding the field of observation, the calculations must be received with considerable reservation. There can be nothing conclusive in the reasoning by which they are brought out, when the phenomena investigated have all the complex and elusive properties of life, blended with and united by sympathies which no human ingenuity can trace, much less determine the amount of their several influences.

Were the doctrine of Magendie and Poiseuille correct, that the tension in all arteries is the same; according to the calculations of Arnott, it would require a weight equal to sixty pounds to arrest the current in the radial artery. The pressure of a few ounces is, however, sufficient for the purpose.

Will it not be admitted, that any given weight, which altogether interrupts the course of a current, is greater than the force by which it is propelled? Poiseuille has regarded the subject in this light, and has employed his hæmadynamometer to determine the impulsive power of the heart. The objections to its use, specified in the course of this inquiry, do not apply in this instance. It has been shown that the instrument destroys, when fixed to an artery, all the forces ordinarily engaged in the circulation, except that of the heart. It is, however, questionable whether these forces be equally annihilated in all cases. The results will be liable to modifications from the size of the animal—the importance of the artery experimented

upon, and the amount of general derangement induced in the animal system. Previously to pointing out these probable sources of fallacy, attention is directed to the following table of experiments, and the accompanying remarks of Poiseuille :

NAMES OF THE ANIMALS.	WEIGHT OF THE ANIMAL.	WEIGHT OF THE HEART.			PRESSURE OBTAINED.
	liv.	liv.	onc.	gros.	Millim.
1. Dog, 1st March	...	0	6	5	148.88
2. Dog, 15th March	...	0	3	7	147.36
3. Dog, 16th March	...	0	3	7	141.45
4. Dog, 19th March	34	0	4	6	157.39
5. Dog, 23d March	42	0	4	1	145.75
6. Dog, 8th April	28	0	5	6	166.60
7. Mare, ... 14th April	...	6	12	d	146.68
8. Horse ... 14th April	...	4	6	d	147.00
9. Dog, ... 25th April	86	0	7	2	179.04
10. Horse, ... 28th April	...	5	d	d	157.25
11. Horse, ... 1st May	...	5	6	d	154.33
12. Mare, ... 1st May	...	3	6	d	182.05
13. Dog, 17th May	19	0	2	6	141.40
14. Dog, 18th May	41	0	6	6	171.14

“In stating the pressure given by the hæmadynamometer, we have not taken into account either the atmospheric pressure or the temperature at the periods of the various experiments ; circumstances which may indeed affect the height of the column of mereury ; but which are, if we may use the expression, null as to the object which we have in view.

“On examining this table with some degree of attention, one is surprised to find that a heart of 3 ounces 7 gros (No. 2.) gives a pressure equal, within a few millimetres, to that given by hearts respectively of 4 ounces 1 gros, of 6 pounds 12 ounces, or of 4 pounds 6 ounces ; and in one no less astonished to find that a heart (No. 9.) of 7 ounces 2 gros gives a pressure greater than that afforded by hearts of from 5 pounds to 5 pounds 6 ounces, of 4 pounds 6 ounces, of 6 pounds 12 ounces.

“The pressures obtained, then, are not in relation to the respective weights of the hearts; hence may we not conclude that the variations observed in the pressures (variations extending only from 140 to 180 millimetres) are attributable to particular circumstances such as age, sex, and state of health—circumstances which we are not in a condition to appreciate? This being the case, are we not right to assume it as a general consequence, *ceteris paribus*, that the pressure given by an artery of an animal of from 300 to 400 kilogrammes, of a horse for example, would be the same as that given by an artery of an animal a great deal less, for instance of a dog of 10 kilogrammes.

“Thus, if we take two arteries of the same calibre, the one in a dog, the other in a horse, in spite of the enormous difference in the weight of these animals, the total forces which move the blood in each of these two arteries will be exactly equal. Further, if considering the adult state of the man, the dog, the horse, we take in each of them an artery of the same calibre, as these arteries are of the same thickness, they are destined, generally speaking, to nourish the same mass of parts. Well, then, there is no reason to think, *ceteris paribus*, that the force which moves the blood in one of these arteries is different from that which moves it in the others. We shall therefore consider these forces as identical.”

He further remarks, “Now, we shall say, since a molecule of blood, taken at any point of the arterial system of man, is moved with a force capable of forming an equilibrium to a column of mercury of a known height, in order to obtain the force which corresponds to an artery of a given calibre, we shall only have to take its diameter; and the weight of a cylinder of mercury, whose base shall be the circle given

by this diameter, and the height that of the column of mercury obtained, will be the total static force with which the blood moves in that artery; that is to say, if a partition were placed there, and held in its place on the side opposite to the course of the blood by a force equal to this weight, the blood would cease to move in the artery.

"We may then establish this general theorem, that the total static force which moves the blood in an artery is exactly in the direct ratio of the area presented by the circle of that artery, or in the direct ratio of the square of its diameter, whatever be the space which it occupies.

"As an application of these principles, let us examine, for example, the force with which the heart propels the blood in the aorta of man, and in some other arteries, for instance, the radial.

"In a man, aged 29, we found the diameter of the aorta, on a level with the sigmoid valves, equal to 34 millimetres under the pressure of 160 millimetres of mercury; we obtained for the area of the circle 908,2857 square millimetres, which, multiplied by 160 millimetres (the height which we take between 140 and 180 millimetres,) give us 145325.72 cubic millimetres of mercury, the weight of which is equal to 1971.77936 gram. = 1.91779 kilogr. or 4 pounds, 3 gross, 43 gr. the estimate of the total static force of the blood in the aorta at the moment of the heart's contraction.

"In a man, aged 46, the aorta under a pressure of 140 millimetres of mercury, gave us 35 millimetres of diameters. Taking the pressure, not of 160 millimetres, as in the preceding case, but of 140 millimetres only, there results 1.828288 kilogr. or 3 pounds, 11 ounces, 6 gros, 4 grains, as the force of the blood in the aorta at the instant of the heart's contraction."*

* Journal de Physiologie, viii., pages 301—305

According to this physiologist, the pressure of the human heart is about four pounds. The evidence, however, on which this rests is not strictly experimental. The height to which mercury ascends in the instrument fixed to the artery of the lower animals, on the contraction of the left ventricle, is the only ascertained fact. In employing it to solve the interesting question—the force exerted by the human heart, the subsequent steps of the investigation are assumed, and consequently the conclusions must be received with considerable qualifications.

He lays it down as an axiom that blood in arteries of the same size, in different animals, is propelled by equal forces. On reference to the table presented by Poiseuille, it will be observed that the pressure of the heart in the dog, No. 14, is 171,14 millimetres, while that in the horse, No. 11, is only 154,33 millimetres, and, with one exception, the dog indicates as great a pressure as the horse throughout the series of experiments.

In the interesting researches of Hales, tubes fixed to any of the principal arteries, in the horse and dog, give very different indications. In the former, blood regularly rose to the height of nine feet, but in the latter seldom exceeded four. In twenty experiments on dogs, in six only did the blood exceed this height, and in five it was considerably below it. The ordinary contractile force of the heart cannot be discovered either by the tubes of Hales or the hæmadynamometer of Poiseuille. If the artery to which either is attached, be large and in the immediate vicinity of the heart, the pressure indicated, not only in this vessel, but throughout the arterial system, will be aggravated, because the heart is excited to inordinate exertion. It is, indeed, stimulated by the sufferings and constrained position of the animal to the greatest degree of contraction.

In confirmation of this, Hales states, that the pulse of the horse, *during the experiment, rose from 36 to 100 beats per minute, the blood at the time being projected with great energy.*

Were it possible to apply the instrument of Poiseuille to a small and unimportant artery, without causing much pain, or placing the animal in an unnatural position, the pressure would be very different from what is recorded. Barry, in his experiments to ascertain the influence of inspiration on the motion of venous blood, states that the coloured fluid in the spiral glass tube employed, scarcely ascended at all towards the chest when the horse was standing, but was drawn with considerable force in this direction, when it was thrown on its back, and held in this constrained position. The same striking difference would be found in the experiments of Poiseuille, if performed under similar circumstances. What, then, is the value of the results furnished by the researches of this physiologist with respect to the propulsive powers of the heart? The extent of these powers, in the undisturbed conditions of the system, is still a problem, the solution of which awaits further and more successful inquirers.

His experiments present data for calculating the extreme force of the heart, but not the influence which it exerts in the ordinary conditions of life. Let not the two questions be confounded. The one, as already remarked, is comparatively simple; the other is complex and difficult of investigation, and will long be one of the mysteries of nature.

The experiments of Poiseuille furnish abundant evidence of the serious derangement produced in all the important functions of the animal system. In its unexcited state, the

two acts of breathing, inspiration and expiration, are so softly performed, that they can scarcely be regarded as affecting the motion of the blood either in the arteries or veins; nor do they, in my opinion, to an extent worthy of appreeiation. In the eleventh experiment, made upon the horse, at one moment, *a*, the column of mereury was not at all affected by inspiration; but in the eorresponding expiration it rose to 180 millim.; at *b*, the column during inspiration rose to 85 millim.; but in the corresponding expiration, it was 95 millim.; at *c*, it was 85 during inspi-ration, and 95 during expiration; at *d*, during inspiration, it was 60 millim., and during expiration, 120 millim.; at *e*, it was 85 during inspiration; and 145 millim. in the subsequent expiration; at *f*, during inspiration, it was 5 millim.; and during the following expiration, 175 millim.; at *g*, the column of mereury was not at all affected by inspiration, but in the subsequent expiration, it rose to 180 millim. Such, then, are the irregularities in the function of respiration, produeed by the researches of the physi-ologist, and eonsequently to the same extent in the circulatory powers, the force of which, in the undisturbed conditions of the animal system, it is the object of his experiments to determine.

If the column of mercury were equally affected by the two acts, inspiration and expiration, this would be no evi-dence of the natural influence of respiration. When the breathing is very much quickened by the pain or suffering of an animal, both acts are exeited, so that the irregula-rities in the pressure indicated by the instrument may be much less than in the instances given, and yet the circu-lation may be disordered to the greatest possible degree. Poiseuille, from observing the difference in the height of the column, during inspiration and expiration, states, that

the force with which the blood is moved in the arteries is diminished during the former act, and augmented during the latter.

This would be the case in his experiments, and whenever the respiration was hurried ; but the fact may be doubted in the ordinary and undisturbed states of breathing. The evidence adduced by Haller, Lamure, Lorry, Magendie, and Barry, establishes the influence of expiration on the current of arterial blood, but only when its action is extreme, and the circulation otherwise unfavourably circumstanced for the due performance of its function. The effect of an ordinary expiration, on the course of the arterial fluid, is in no degree elucidated by the labours of these distinguished physiologists. "We infer," remarks Poiseuille, "that the influence of the motions of respiration upon the circulation of the blood is increased in the large as well as in the small arteries when violent respiratory efforts succeed ordinary efforts. But this influence, particularly in the large arteries, is such, that, in inspirations, the force which moves the blood is very nearly equivalent to nothing, if it be not really nothing ; and in compensation in corresponding expirations, this force becomes almost twice as great as in the natural state." The circulation is not thus circumstanced in the ordinary conditions of the system. In the experiments of Poiseuille, the arteries are full to distension, and in those in which the hæmadynamometer is fixed, the blood, as previously remarked, has no means of escape, and, consequently, the agency of the heart and that of respiration, will be represented by the instrument in its most aggravated form.

The effect produced upon the circulation by either power is no indication whatever of the manner in which it is influenced in a natural state of the animal system, nor can

any just inference be drawn from it concerning the motion of the arterial fluid during the two acts of respiration. In my opinion it is neither accelerated nor retarded by them, in their unexcited state. Expiration, when violent, urges the contents of the arteries, simply from the contraction of the abdominal and thoracic parietes. In inspiration, the fluid may at times appear to be at rest, merely from the suspension of such contraction, and not from any suction power of the chest. Poiseuille remarks, that the influence of inspiration and expiration is the same on arteries differently situated with respect to the heart.

The instrument will unquestionably furnish uniform results. The only difference in the column of arterial blood near to, or remote from the heart, in the experiments of Poiseuille, is in its length, which cannot materially modify the agency of expiration. If he had considered that the blood, extending from the instrument to the heart, is confined between these two points, and that the vessel in which it moves, as well as the rest of the arterial system, are extremely distended, this uniformity of pressure would have been anticipated.

To torture an animal, and at the same time to obstruct the circulation in a principal artery, is to produce extensive derangement in the action of the heart—in the function of respiration, and, as a necessary consequence, throughout the whole of the sanguiferous system. The arteries, the capillaries, and the veins are equally implicated in the induced disorder. These vessels are not to be regarded as possessing always the same quantity of blood, and therefore having certain unalterable relations to each other in every state of the animal economy. Their contents are constantly varying in amount. At one time, the

arteries contain an exceedingly small quantity, as is obvious from the scarcely perceptible pulse; at another, they are full to distension, and the fluid is urged forward with great force.

The capillaries and veins will be affected contemporaneously, and to an equal degree, by the same general interference, and the modifications induced will co-exist with corresponding changes in the action of these vessels, as well as in other powers instrumental in the motion of the blood—changes which no sagacity can estimate. These remarks apply as forcibly to experiments to determine the office of the capillaries or the veins, as that of the arteries; and, in a subsequent part of this inquiry, it will be shown that physiologists, neglecting to take into account the serious disturbances flowing out of these experiments, have reasoned inaccurately on the phenomena of circulation.

When the contractions of the heart become weak, the column of venous blood augments,—a circumstance exceedingly unfavourable to the views of those who contend for the direct and all-important agency of this organ. It is admitted by Magendie that, in this state of the animal system, he failed to arrest the current flowing from the punctured vein, on intercepting the impulse of the left ventricle in the corresponding artery. The fact is altogether irreconcilable on the doctrine, that the contents of the veins are propelled by this power. If the cause of venous circulation, why did the blood continue to escape when manifestly withdrawn from its influence?

The changes in the distribution of the vital fluid, under peculiar circumstances, may be regarded as a wise provision in nature, securing life from distressing and perhaps, fatal accidents,—a provision which could not have been suggested on the prevailing views of writers.

The mass of blood to be moved diminishes with the enfeebled actions of the heart; and, were not this the case, death would frequently be the inevitable result. In illustration of this truth, I may advert to the modifications in its distribution in syncope, during which the heart is virtually quiescent. At this time there is a gradual diminution of blood in the arterial system, and a proportionate augmentation in the venous. On the return of sensibility, the contractions of the heart are extremely slow and feeble; *but the small quantities of blood propelled meet with no obstacles in their course towards the capillaries.* Can these contractions be conceived capable of communicating an impulse to the accumulated contents of the veins? They are certainly inadequate to the production of such effect. When this impulse was interrupted in the experiments of Magendie, the blood beyond the point of compression flowed into the capillaries and veins, leaving the artery empty. If this be the condition of the venous system during syncope, and the fact is undoubted, it will scarcely be urged that the reviving contractions of the heart propel the mass of blood in the veins, which is nearly at rest.

It has been remarked by a distinguished writer, "that when death is the consequence of arterial hemorrhage, the venous system remains gorged with blood, which proves that the impulse of the heart is necessary even for the venous circulation."* Such condition does not establish the fact. Death arising from hemorrhage is always preceded by syncope, which arrests the action of the heart, and consequently the escape of blood. In all cases of dissolution,

* Paper read by M. Amussat, before the French Academy of Sciences, February 1843. See London and Edinburgh Medical Journal, April, 1843.

this leaves the arterial and accumulates in the venous system. The general results are the same, whether death be occasioned by external injury or the slow process of disease.

On the cessation of the heart, the blood in the arteries flows into the capillaries, and these convey it into the veins, producing engorgement according to the quantity in circulation. Had the arteries, the capillaries and the veins, either separately or conjointly—the power of propelling the fluid, where could it move to? The heart is no longer in action to give it a passage, and therefore, the congestion of the veins is a necessary result.

It has been proposed to measure the force of the heart by an experiment, which has been regarded as exceedingly simple, and which is often referred to by physiologists. It consists in crossing the legs, and placing upon one knee the ham of the other leg, with a weight of 55 pounds appended to the extremity of the foot. This considerable weight, though placed at the extremity of such a long lever, is raised at each contraction of the ventricle, in consequence it has been supposed, of the tendency to straighten the accidental curvature of the popliteal artery. A writer, who has displayed considerable originality in physiological pursuits, remarks with great truth on this experiment, “in supposing that the force of the heart moves the extremity of the toe, loaded with a hundred pounds, they have forgotten that the weight of a few pounds will stop the pulsation at the groin. If they had thought of the tying of an artery with a hair, or of the compression of it with the point of the finger, or of the delicacy of the valves and coats, would the force of the heart ever have been estimated at 180,000 pounds, or indeed at 55 or 100 pounds?”

“The calculation of the force exerted by the heart, in raising a weight upon the foot, has been a favourite theme ; and if we could admit that the power of the heart was equal to the effect, we should be forced, perhaps, to admit the extravagant conclusions which have been drawn from this experiment. But it is altogether a delusion. I shall first state the fact and the form of the argument.

“When one leg is twisted over the other knee, and the foot is held suspended, a distinct pulsation is perceived in the toe, and the toe will pulsate and rise, although the foot be loaded with 100 pounds. Now here the whole power of the heart cannot be exerted, for its force is divided between the head, the arms, the legs, so that, at the most favourable calculation, it is a fifth part of the natural force of the heart which moves the leg. In the next place, the leg is here as a lever of the third kind ; the moving power is the artery behind the knee-joint ; it moves the weight upon the foot in proportion as its distance from the centre of motion in the knee joint is less than the distance of the centre from the weight, how much, therefore, must the weight of 100 pounds be increased in its pressure upon the pulsating vessel ? yet the vessel throws it up ;—the heart distending the vessel, with but a fifth part of its power, throws up this accumulated weight ;—truly, then, the force of the heart is beyond calculation. What must it be at the aorta ? What can withstand it ? This consideration alone should have led to the re-examination of the presumed data, the grounds of this reasoning.

“First, then, we find that at most a few pounds will stop the pulsation of the crural artery at the groin. In the next place, we find that, if the knee be supported upon a small pad, placed betwixt the ham-strings, or if the knee be suspended by grasping the condyles of the femur, there

will be no pulsatory motion of the foot, although the leg hangs at right angles with the thigh. Farther trials will show that the fleshy bellies of the muscles of the calf or ham must rest upon the knee to produce the effect contemplated. If the experiment be made with the arm, the same circumstances will be observed. When we rest the elbow on the table, and bend the fore-arm to a right angle with the arm, no pulsation will be seen; but if the flesh of the arm rest on the back of a chair, so as to compress the veins, and make the bellies of the biceps and triceps press against the support on which the arm is extended, then the hand and fingers will pulsate like the foot in the other instance. But if the arm be made to press in such a manner that the trunk of the artery is compressed, and not the bellies of the muscles, then there is no pulsation observable in the hand and fingers. It follows that we are wrong in supposing that the motion of the foot is owing to the popliteal artery changing from the curved towards the straight line; or, in supposing that it is the attempt of the current of blood to force itself against the compressed artery. It is not the popliteal or the brachial artery which throws up the limb; it is the pulsation of the smaller arteries within the fleshy substance of the limb that produces the motion.”*

The circulation in a principal artery being easily arrested by the pressure of the finger, it is absurd to imagine that the impulse of the left ventricle is equal to 50 or 100 pounds. The experiment, which is supposed to establish this fact, I modified in the following manner:—Weights were successively placed upon the fleshy part of the thigh of an athletic individual, immediately over the crural artery, until nearly 20 pounds had been applied.

* An Essay on the Forces which Circulate the Blood. By Sir Charles Bell, F.R.S.E.

This, however, was not sufficient to stop entirely the pulsation of the artery. The mass of flesh on which the pressure is made, is composed of numerous capillaries and of muscular fibres, both of which struggle to regain their natural position, and according to the degree of the reaction, is the ease with which the impulse of the left ventricle elevates the super-imposed weights.

It is remarked by Bell, that "it is not the popliteal or the brachial artery which throws up the limb; it is the pulsation of the smaller arteries within the fleshy substance of the limb that produces the motion." His explanation is not altogether satisfactory. In my opinion the muscular fibres perform as important a part as the capillaries. When it is considered that the muscles are endowed with elasticity, or a property in virtue of which they return to their original form, on the removal of external pressure, and in the effort to overcome it, diminish the actual weight upon the artery, it is not extraordinary that this should appear to exert such power. The weights are not moved synchronously with the impulse of the left ventricle, by the reaction either of the capillaries or the muscles, but by the artery. The reaction of the muscles and of their contained blood vessels, affects the weights in the same manner, as if an attractive force drew these from the limb, so that the impulse of the heart has not to raise a dead weight of so many pounds, but the amount which remains after deducting the influence of such reaction. What this may be it is difficult to ascertain.

Did the blood flowing along an artery possess the force ascribed to it, the experiments of Poiseuille, in which the impulse of the left ventricle is impressed upon a column of mercury, would establish the fact, and yet the results are widely discrepant. In his procedure no calculations are

necessary concerning the agency of either muscles or capillaries. The left ventricle has free play to act upon an obstructing column, and the effect produced upon it will be a measure of the impelling power.

In the preceding investigations, it has been shown that the force of the human heart cannot be deduced satisfactorily from the experiments of Poiscuille. It appears to me, however, that his method makes a nearer approach to the truth than the attempts of any other inquirer. It is impossible to apply an instrument capable of determining with precision the impulse of the heart, or the co-operation of the several organs engaged in the circulation, without disturbing or annihilating their respective functions. In conclusion it may be remarked :

1. That the prevailing defect in the researches of physiologists concerning the powers which move the blood, is to be traced to the little attention which has been given to the natural conditions of these powers, and their important relations to each other. In experiments to ascertain the force of the heart,—the influence of the arterics, or the effect of respiration, the derangement produced in the objects investigated has led to no practical or speculative considerations.

2. I have dwelt, at considerable length, on the celebrated experiment of Magendie. The imagined conclusiveness of it, conjoined with his high authority, gave it a just claim to especial notice. If the strictures, however, be well founded, it will no longer be appealed to as an illustration of the dependence of venous circulation on the direct action of the heart. It has, also, been stated that syncope is accompanied by changes in the condition of the arteries, the capillaries and the veins, the same as occur in this particular experiment, and consequently it furnishes no corroborative evidence.

3. The experiments of Poiseuille, do not give the exact force of the human heart in the ordinary conditions of the animal system. The instrument, nevertheless, is better calculated to elucidate the subject than any of the various methods previously employed. As already remarked, its peculiar fitness to measure the impulse of the left ventricle renders it altogether inapplicable to determine the energy of the circulation in different arteries.

BOOK IV.

THE FORCES BY WHICH THE BLOOD IS MOVED IN CAPILLARY VESSELS.

CHAPTER I.—THE PREVAILING OPINIONS ON THE CHARACTER AND VARIETY OF CAPILLARY VESSELS.

The inquiry concerning the functions of the capillaries is, indeed, exceedingly complex, necessarily including the investigation of several powers, all more or less associated in the production of one effect—the circulation of blood. In the attempt to analyze the functions of the capillaries, it is essential that the physiologist should have clearly defined ideas in regard to the amount of co-operation derived from the heart, the arteries, the veins and the alternating changes of the chest.

Some of the earlier physiologists, especially Whytt and Cullen, ascribed to these vessels a much greater influence than is accorded to them by the moderns. It will, however, be my object to show that the views of the former are, in a great measure correct; and are alone adequate to explain

many of the phenomena of circulation. After detailing the opinions which are, or have been entertained respecting the character and variety of capillaries, I shall pass to the examination of the numerous experiments, imagined to prove that these vessels are not endowed with contractility, and in no degree facilitate the motion of blood.

The exceedingly minute vessels which require the aid of the microscope to discern them, have been designated capillaries. They are found abundantly, but not in equal proportions, in every part of the body, and some writers have considered the animal structure as entirely composed of them.

The capillaries are the last divisions of the arteries, and the attenuated roots of the veins, connecting directly the arterial and venous systems, as is proved by injections, and the examination of capillary circulation in the field of a powerful lens. In the former case, the injection passes from the larger arteries into the larger veins; and in the latter, the blood is seen to move slowly, but continuously through the net-work of minute vessels. The direct connexion of the two systems, by means of intermediate capillaries, is a point that can scarcely be said to admit of dispute. It has, however, been called in question by two modern physiologists. M. Doellinger imagines that the arteries, in their last divisions, have no parietes, and that the blood moves, destitute of such, through a peculiarly organized matter, which he calls mucus. One part of the vital fluid is considered to be converted into this, and the other is said to pursue its course, passing into capillary veins and lymphatics which arise from this substance, in the same manner as the arteries terminate in it. M. Wilbrand advocates a still more extraordinary doctrine. He supposes that the whole of the blood during its circulation

is converted into organic matter, mucous substance or secreted fluids ; and that a proportionate quantity of blood and lymph is contributed, or thrown off by the different organs ; and hence the circulation is maintained as if the arterial and venous systems were directly connected. Both assert that there is no evidence derived either from injections or the magnifying power of the microscope, clearly establishing the direct termination of arteries in veins. The fact, however, cannot be called in question.

The capillary vessels differ greatly in size. Some admit several globules abreast, others only two or three ; and some are so extremely fine that they receive only one globule. They all anastomose extensively with each other. Though it is not possible to determine, from a knowledge of their structure, the vital properties with which they are endowed, the study of their functions, under different circumstances, will facilitate the inquiry, and dissipate much of the obscurity in which the subject is involved. The capillary artery and vein may contain either red or colourless blood, according to the size of the vessels and the nature of the organs or tissues in which they are distributed.* Haller, and the physiological authorities succeeding him, endeavoured to prove that an artery terminates in one of the following modes :—

1. Either directly in a red vein or veins. 2. In excreting ducts. 3. In exhalants. 4. In lymphatics, 5. In the colourless artery.

The existence of these different classes of vessels carrying the colourless part of the blood, is advocated by Boerhaave, Vieussens, Ferrein, Soemmering, Chaussier,

* Elements of General and Pathological Anatomy, page 136 ; by David Craigie, M.D. A work replete with much valuable information.

and the majority of anatomists and physiologists of modern times. The opinion rests on microscopical observations, injections, and the changes occurring in white and transparent tissues during inflammation. Vessels which in health carry a colourless fluid are at this time strongly injected with blood.

Prochaska, Mascagni, Richerand and others deny the existence of these vessels. It is, however, probable that arteries have other terminations than in veins, though there is no proof but what is liable to objections.* The variety of vital actions seems to demand a more extensive circulation than the ascertained and acknowledged channels of supply.

Many ingenious views have been proposed to account for secretion, absorption, and nutrition ; but whether these functions be effected in peculiar vessels, or in the ultimate divisions of the arteries, is yet undetermined. The important point to which the attention is solicited by these introductory remarks, is simply that arterics terminate directly in veins ; a conclusion almost as unquestionable as the circulation of blood. Such connection existing between the arterial and venous systems, it will readily be conceived that whenever the circulation is disturbed in the larger arteries and veins, the intermediate and associating net-work of vessels will, also, necessarily be involved in the derangement. Were these entirely passive, they would be affected by every modification in the flow of blood to, as well as by every impediment to its transmission, from them. If, however, endowed with vital and active properties, the functions which they exercise, cannot possibly be carried on, as in health, when any part of the sanguiferous

* See the opinions of Magendie on the Lymphatics.

system is disordered by the reckless interference of the physiologist. To increase or diminish the supply of blood to them, or to retard its flow in the larger veins, will produce serious changes in their condition.

Results obtained under such circumstances, in investigations to determine either the course of the blood in the capillaries, or the influence of these vessels, are, as will subsequently be shown, open to formidable objections. No experimentalist has taken these modifying causes into consideration, or has appeared even sensible of their existence. Whether the object was to ascertain the propulsive force of the heart, the action of the capillaries, the condition of the veins, or the alternate changes in the capacity of the chest, on the motion of the blood, in no case was there any calculation of the amount of the disturbance induced by the direct interference of the physiologist. In analysing the labours of Magendie, no expression occurs in contradiction of this broad and unqualified assertion, but, on the contrary, copious evidence, illustrative of its truth

CHAPTER II.—AN EXAMINATION OF THE EXPERIMENTS WHICH HAVE BEEN PERFORMED TO ELUCIDATE THE FUNCTIONS OF THE CAPILLARIES.

The forces on which the motion of the blood depends, according to some writers, are four—the heart, the arteries, the capillaries, and the agency of inspiration, which are imagined to co-operate in very different degrees. Others assert that it is by one or two only of these powers that the blood is urged forward. If, therefore, the intention is to ascertain the exact influence of any one, it is evident the results will be imperfect, and liable to objections, if the natural relations of these powers be disturbed in the inquiry.

If we wish to estimate the action of the heart, or the extent of its co-operation in the circulation, the means employed should not at all interfere with the conditions of the other powers. The arteries and capillaries must receive neither more nor less blood than usual. In either case they may be incapacitated from acting; or the results observed will be no evidence of their ordinary influence. The larger arteries are considered to contract or return upon their contents with a force equal to that by which they have been distended; but, if they receive more or less blood than is compatible with the proper exercise

of their functions, the experiment will at once be admitted to be imperfect, because in attempting to measure one force, it has annihilated or disturbed others.

In illustration of the justness of these remarks, I shall again recur to the celebrated experiment of Magendie. No other in this department of physiology is equally well known or adapted to the purpose. In studying the labours of others, in reference to the functions of the heart, the arteries, the capillaries, or the veins, it is constantly alluded to ; and, consequently, in the attempt to trace the office of the capillaries or veins, it is again necessarily brought under review. The conclusions drawn from it are not false only with respect to the arteries, but equally so with respect to these two classes of vessels. It is indeed a comprehensive fallacy, pervading the whole of the reasoning of this enterprising inquirer, and has been implicitly adopted by the highest authorities. The experiment is thus described in his own words.

“After having passed a ligature round the thigh of a dog, that is, without including the crural artery or vein, apply a ligature separately upon the vein near the groin, and then make a slight opening in this vessel. The blood will immediately escape, forming a considerable jet. Then press the artery between the fingers, to prevent the arterial blood from reaching the member ; the jet of venous blood will not stop on this account ; it will continue some instants ; but it will become less and less, and the flowing will at last stop, though the whole length of the vein is full. If the artery be examined during the production of these phenomena, it will be seen to contract by degrees, and will become completely empty. The blood in the veins then stops ; and, at this period of the experiment, if you cease to compress the artery, the blood injected by the heart will

enter, and, as soon as it has arrived at the last divisions, will begin to flow again at the opening of the vein, and by little, and little, the jet will be established as before."

This is imagined to prove that the impulse of the heart is transmitted throughout the circulatory system, and further, establishing that the motion of the blood is not influenced by, or at all dependent on the capillaries. It is, in my opinion, one of the least satisfactory experiments to be found in the whole range of physiological researches. The analysis of it will show that it possesses none of the conditions necessary to ensure the attainment of the object contemplated. The intention is to ascertain whether the motion of blood depends on the heart alone. This is the problem to solve. Every inquiry, disturbing the relations existing between the functions investigated—co-operating in the production of the same effect, will be imperfect and liable to objection. Does this experiment leave untouched the natural relations of the heart, the arteries, the capillaries, and the veins? It deranges, indeed, the whole series. The arteries cannot act on the vital fluid, as in the ordinary states of the animal system; nor can the capillaries, for in one stage of the experiment they are actually not supplied with it.

On making a slight opening in the vein, the blood immediately escaped, forming a considerable jet. If the artery, at this moment, however, be compressed, so as to cut off the influence of the heart, "the jet of venous blood will not stop on this account; it will continue some instants." It is here distinctly admitted that the blood flows from the vein when it is not under the influence of the heart. According to his own reasoning, it ought to have ceased flowing almost immediately after intercepting the impulse of this organ. It is further observed, the "jet

will become less and less, and the flowing will at last stop, though the whole length of the vein is full." The gradual diminution of the jet would occur, were the capillaries the only force to which venous circulation could be attributed.

The blood escapes, according to his own account, until the artery pressed upon is completely empty ; in other words, the jet is not arrested, as long as any fluid is transmitted to the capillaries. He deprives these vessels of blood, and then, when clearly incapable of furnishing a supply to the vein, necessary to maintain the jet, he regards the phenomenon as unequivocally demonstrating that the motion of blood in capillaries and veins depends exclusively on the action of the heart. It is remarkable that one so familiar with experimental researches, and who has laboured with assiduity to prove that many of the phenomena of life, and especially those of circulation, are alone explicable on mechanical principles, should have neglected altogether the study of the induced conditions of the vessels investigated. From the character of his mind and the tendency of his pursuits, one would have expected him to have been keenly alive to the disturbing causes of the experiment.

The cessation of the jet is no evidence that it arises from interruption to the influence of the heart. The same result would occur were the capillaries the cause of venous circulation. The stream flows as long as the capillaries receive blood. If none exist between the roots of the vein and the compressed artery, whence is the power to be derived to act on the contents of the vein ?

In the attempt to prove that the venous current is propelled by the heart alone, in common justice to the capillaries they ought not to be deprived of blood. It is unphilosophical to withhold this, by which they are stimulated to contraction, and then to accuse them of the

want of power. Were the physiologist to prevent the blood flowing into the left auricle, and observing the arteries full to distension, to call attention to the fact, as evidence that the contents of these vessels were not at all influenced by the heart, the inference would naturally be objected to. But the capillaries are placed precisely in this situation. There is no difference between them and the heart in the case supposed. The artery, from which they receive their stimulus and source of contraction, is empty, and, consequently, the supply is cut off. There is a column of blood in advance of them, but there is none, *a-tergo*, by which to act upon it.

It is stated that when the jet stops, "if you cease to compress the artery, the blood injected by the heart will enter, and as soon as it has arrived at the last divisions, will begin to flow again at the opening of the vein, and by little and little, the jet will be established as before." This is in direct confirmation of the objections urged against the explanation of the physiologist. The jet is not re-established until the artery and the mass of capillaries have received an abundant supply, and even then it is not formed at once, but by degrees. This is not consistent with the views and reasoning of Magendie.

The blood does not escape until that conveyed by the artery reaches the last divisions, that is, until the capillaries are distended, and even when there is a continuous column extending from the punctured vein to the artery, the contraction of the heart does not immediately re-establish the jet. If the blood move only from the impulse of the heart, and this be freely transmitted, such ought to be the case. It is evident, from the latter part of the extract, that the capillaries, previously to the finger being removed from the artery, are quite empty.

So far from the experiment demonstrating that the motion of blood in the veins depends on the heart, it may be used to prove the reverse of the argument, viz., that venous circulation depends mainly on the capillaries. The jet is admitted to continue after the impulse of the heart is wholly intercepted; indeed, as long as there is any blood in the artery and capillaries. How is this to be reconciled with the doctrine, that the blood is moved only by the heart? The manner in which the jet is re-established, is in part explicable on the gradually improving condition of the capillaries. These vessels cannot at once be restored to vigorous action by the influx of arterial fluid. At first this action is feeble, and the influence exerted on the venous column proportionably weak, but this increases in strength as the vessels acquire their natural tone and energy.

The same experiment has recently been performed by Magendie, and attended with circumstances of peculiar interest. On the repetition of it, the blood flowed from the vein without any jet, or "the ordinary effects of the heart on the circulation being witnessed." This he attempts to explain, by stating that the animal was weak from the previous injection of water into the veins. Is it then to be inferred, that the venous system is influenced by the successive contractions of the left ventricle only when the vital powers are vigorous?

A subsequent experiment, performed on a healthy animal, and which deserves particular attention, is thus described:—

"But to return to our experiment. We chose a much stronger dog than the last, and took care to avoid everything which might influence in an unfavourable manner the production of the wished-for result, for these experiments are difficult, and require a great deal of caution and

management ; the crural vessels of the hinder leg were exposed ; here there are the artery and vein running close to one another ; the main nerve was cut away. If the parts be left in the condition in which you now see them, we shall not be able to obtain the proper phenomena, because the collateral branches would constantly furnish an intermediate current of blood ; it is, therefore, necessary to pass a circular ligature round the limb, above the point where the vessels are exposed, but not embracing them ; by this means the circulation in the leg will be confined to the femoral artery and vein. Having taken this necessary precaution, I now place a ligature on the femoral vein, and make a small opening through the parietes of the vessel ; the jet of blood is, as you see, very strong, though small, *but the alternate impulses of the heart are not marked by the alternate elevations of the stream* ; of this you already know the reason. I now take the artery between the forceps, and compress it a little ; the jet diminishes at once ; I increase the compression, and the stream continues to fall ; it is lower, lower, lower, and now it has ceased altogether. When I withdraw the forceps, and give free liberty to the artery, the force of the heart is permitted to be felt, and the stream immediately recommences to the same extent as before ; when, on the contrary, I press on the parietes of the artery, the jet, which was more than a foot in height, falls, as you see, to a few lines ; it does not cease altogether when the cavity of the artery is obliterated, and the passage of blood through it is consequently stopped ; for, the venous system being full of fluid, and endowed with a high degree of elasticity, continues the jet by its proper action for a short time after the motion communicated by the heart has been interrupted."*

* The Lancet, 1834, p. 407.

He admits that, when the vein is punctured, the blood which is projected is not marked by the alternate impulses of the heart ; and further, that the jet continued when the impulses of the left ventricle were no longer communicated—an effect which is ascribed to the distension of the veins, and the elasticity with which they are endowed. The only evidence in support of his view is, that the stream flowing from the vein is more or less modified by the condition of the artery. When the blood is allowed to pass freely through it, the current projected from the vein is strong. When intercepted by the pressure of the finger, it is correspondingly weakened.

The stream will unquestionably be modified according to these circumstances, and such might have been anticipated independently of all researches. To illustrate the nature of the objections which present themselves to the mind, in the consideration of this experiment, suppose that two individuals look upon a stream of water exposed, at two points only, to the day, the space intermediate being concealed from view. At one point it is seen to pass beneath the ground and is lost ; at another, it is perceived to issue from it. This is precisely the case of the arteries and veins in the experiment in question. The blood is seen to pass through the one and issue from the other. The space between the two is occupied with minute vessels, which do not fall under observation, and thus it may not inaptly be regarded as the ground which hides the circuitous meanderings of the arterial fluid. The two individuals might possibly dispute concerning the causes by which the motion of the water is influenced. One might insist that the stream which issues again into day is urged forward by forces acting upon it previously to disappearing from sight ; the other might assert that it was modified by

circumstances existing between its entrance and exit. To settle the contention, one proceeds to make a bold and exceedingly original experiment. Regarding the other with that confidence which a sense of superiority usually inspires, he exclaims, "You will see that when the water which enters the ground is diminished, the quantity which escapes from it will be correspondingly lessened. If none is allowed to enter, none will issue. Can you possibly desire a more conclusive experiment?" The other might justly reply, "The experiment proves nothing but what might have been anticipated. I admit that the stream which escapes from the ground will be proportionate to that which enters; and this is all that the experiment confirms. The circumstances imagined to exist between the two points cannot act unless they have something to act upon. Nor do the changes in the strength of the current escaping, in harmony with the modifications previously induced in it, disprove the influence of these circumstances." The experiment of Magendie establishes simply the same fact. The stream which flows from the vein varies according to the quantity of blood allowed to pass through the artery. How can it possibly be otherwise, if a direct connexion exists between arteries and veins? The jet is described as falling from a foot to a few lines, when the pressure of the finger arrests the passage of the stream through the artery. This is quite natural. How can the height of a foot be maintained, if the vein is not supplied with blood? It would be absurd to calculate on the water-wheel continuing to exercise its power when the course of the stream is diverted.

The jet is not arrested when the action of the left ventricle is intercepted; and yet the doctrine taught is, that the blood in the vein moves only from the direct im-

pulse of the heart. The physiologist endeavours to get over this difficulty, by the aid of two suppositions—the fulness of the venous system and its high degree of elasticity. The mere fulness of a vessel cannot be a source of motion. The elasticity of it comes into play on the cessation of the cause which urges the fluid *a-tergo*, but it is altogether incapable of causing a series of successive contractions. To argue for such an influence is to assert that the vein has within itself a power like that of the heart, by which the fluid is transmitted regularly in its course. If endowed with this power, how is it that, when the jet has altogether ceased, the vein is full; a condition which is admitted, and brought forward in support of the direct action of the heart? If the vein possesses such power, why does it remain distended? According to his own reasoning, if the fulness and elasticity of the venous system are capable of producing the jet, why does he not maintain these conditions in the attempt to measure the influence of the heart? So far, however, from deeming such step necessary, the influence is intercepted in the artery, so that the mass of vessels between this and the vein are no longer in a state of distension, or supplied with blood, and then, because the jet ceases, he presumes to have proved that the capillaries play no important part in the process of circulation. Every interference which disturbs the natural condition of these vessels, giving them either more or less blood than they receive in the uninterrupted actions of the system, invalidates the experiment. The phenomena evolved, under these circumstances, are not the ordinary language of nature, and to interpret them as such is to disregard the sound rules of philosophy and of common sense.

This physiologist, by another experiment, seems to ima-

gine that he has for ever set at rest the question of venous circulation. It is thus described by him, and, in the consideration of it will be found an admirable illustration of the justness of these strictures :—

“I now proceed to make an experiment which is even more decisive than the former. The impulse of the heart will be replaced by artificial means, and you will witness analagous effects. I now push a quantity of warm water into the femoral artery, and the jet from the vein is immediately increased in proportion. I cease to act on the piston. The stream falls considerably, as you must remark. This is done several times, and the result is always the same. The more forcibly I inject the warm water, the more perfect and rapid is the jet from the vein. Were the animal larger, you would see this much better. What you have witnessed, however, is sufficiently convincing, for nothing, in my opinion, can prove more directly the easy and immediate passage of fluid from an artery to a vein, and the manner in which the contraction of the heart produces this effect in the living subject.”

Should this experiment, and the theory with which it is associated, be transmitted to distant times, it will be brought forward as evidence of the little progress of physiological science in our day. It will stand out boldly, enunciated by a high authority, and implicitly received by contemporary and master spirits of the age. Comments on such an experiment are scarcely necessary. The physiologist, to prove that the blood in the veins is propelled entirely by the heart—distends with water the mass of vessels between the artery and the vein, and, observing that it escapes from the one as it is injected into the other, the result is seized with avidity as an illustration of the direct action of the heart.

The stream which escapes, ebbs and flows according to the varying strength and quantities of the injection, and were it not so the discrepancey would be a phenomenon,—an exception to the laws of physies, and would be more difficult of solution than the functions of the capillaries. Let us pause to examine the condition of the several classes of vessels in this experiment, and to point out their wide departure from those which exist in the natural states of the animal system. This inquiry, as already remarked, has a value beyond the exposition of a single fallacy. It brings under consideration the phenomena of life, and endeavours to facilitate the study of them by exhibiting errors in important investigations.

I. The arteries, the capillaries, and the veins, are never full to distension in any condition of life. It is an impossibility, and would be incompatible with existence. In these circumstances, one propulsive power only could act. The distribution of the blood is constantly varying. At one time the veins contain the greater portion in the body. At another, the arteries are more than usually supplied. Were both these vessels and the capillaries in the condition which is produced by the injection of water, such changes could not occur.

II. Not one drop of water issues from the vein until the vast series of vessels between it and the artery are full to distension. Every ounce of water afterwards injected, must displace a corresponding quantity. Neither the arteries, the capillaries, nor the veins can, at this time, exercise the slightest influence. They become as inorganic tubes, and therefore in what way does the experiment elucidate the phenomena of circulation? What light does it throw on the functions of these vessels, when their vital and physical properties are alike incapable of action?

III. To determine the functions of the capillaries, these must be supplied with blood, and not water, and they must receive it in such quantities as nature alone can regulate. The physiologist must not presume to measure it. He knows nothing of their varying necessities. When he flatters himself that he understands them, and attempts to imitate the delicate and susceptible powers by which the supply is beautifully adjusted to the demand, he forgets the duties and the character of the philosopher.. He cannot penetrate the secrets of nature, except by studying the phenomena which she places under observation. When he mutilates an animal with his scalpel, dividing artery from artery, and nerve from nerve, regardless of all vital laws and principles, the answers which he wrings more frequently mislead than open upon the mind enlarged views and comprehensive truths.

The false facts of physiology, rivetted upon the understanding by experiments, are the difficulties opposed to the sound progress of the science. In our own day and country there is, however, one physiologist standing out from the multitude, that groped not after truth in the midst of perplexities and confusion, allowing experiment to precede profound reflection. The strictly philosophical method which he pursued will be as valuable to succeeding inquirers as the originality of his discoveries. He, almost alone, has treated nature with delicate considerations.*

Though it is fully established that arteries terminate in veins, it is not to be inferred that this is their only termination, or that capillary vessels at present conjectured rather than proved to exist, may not be constantly pouring their contents into the venous system, by which

* Sir Charles Bell.

the circulating fluid may be modified, both in quality and quantity, That the blood in its complicated circuit, acquires and loses a variety of elements, is a fact which needs no confirmation. The nature of what is thrown off in the minute vessels, or converted into organic structure, and the manner in which these processes are effected are amongst the mysteries of science.

The experiments of Magendie take into consideration none of the modifying circumstances of vitality; and in the rashness of his procedure, conditions are induced which prevent the exercise of physical properties, which he regards as the cause of the phenomena of circulation.

In the experiment, in which the jet is emitted from the vein, with more or less force, according to the current flowing along the artery, in one stage of it, the intermediate mass of vessels will necessarily be distended or placed in unnatural circumstances. When the stream ceases to escape, the vein is admitted to be full, so that here is a column at rest, and cannot possibly be moved until the numerous vessels, between this point and the compressed artery, are in a state of distension. At this moment there is no blood between the vein and the artery. It passed from the latter to the former after the influence of the heart was intercepted; and consequently the contents of the vein remain quiescent until the current of the liberated artery has filled the intermediate space. In this case there will be a column extending from the artery to the punctured vein, and what is to prevent the heart urging it forward, precisely as the stroke of the piston acts upon the vessels distended with water?

In the undisturbed state of the animal system, such is not the condition of the arteries and veins. The ligature which is passed round the thigh of the dog confines the circu-

lation to the crural artery and vein, and the intermediate mass of vessels connecting the two. The artery receives no supply from lateral communications, nor can the vein transfer a portion of its pressure to branches by which it may be equalized. It is, therefore, evident that the heart acts upon a diminished column of blood, and which is so narrowed in its sphere of motion, that the vessels in which it moves will clearly be in a state of forced distension, when the jet is projected from the vein. Under these circumstances, the arteries, the capillaries and the veins, cannot bring into play the influence they are admitted to exercise on the course of the blood in the ordinary conditions of the vital powers.

The experiment throws no light on the functions of the capillaries. At one moment it deprives them of blood, and then the attention is drawn to the fact, that they do not urge forward the venous column; at another they are filled to distension, so that the impulse of the left ventricle is transmitted throughout this limited sphere of circulation, like the stroke of the piston on the injected fluid. The physiologist, contemplating these results, flatters himself that he has established the justness of the application of physical principles to the explanation of vital phenomena.

The direct influence of the heart is also inferred from observing the effects of depletion and fainting, on sanguinolent secretions and hemorrhage. This subject has already been slightly touched upon, but I again recur to it, as the consideration of it properly belongs to this branch of the inquiry. Fainting makes a powerful impression upon the circulation, almost at once arresting any unnatural discharge; and the phenomenon certainly cannot be adduced as evidence that the capillaries do not co-operate in an important degree in the motion of the blood.

Syncope, whether caused by mental impressions from objects without, or by depletion, occasions either cessation or imperfect action of the heart. In this case, the blood in the different series of vessels is circumstanced as in the experiment of Magendie, in which the artery is compressed, intercepting the impulse of the left ventricle. It passes from the arteries into the capillaries and veins. It cannot flow in the direction of the right side of the heart, the right auricle and ventricle being at rest.

Five remarkable conditions accompany syncope:—

1. The blood leaves the larger arteries, and accumulates in the capillary and venous systems.

2. The blood is as strictly confined within these two systems as if a ligature were passed round the *vena cava*.

3 The capillaries and veins are not at all influenced by the movements of respiration.

4. The arteries are incapacitated from acting, receiving no regular supply of blood from the heart.

5. The blood is rendered less stimulating in its qualities.

It may possibly be urged, that fainting usually continues too short a time to cause such effects. In answer to this objection it may be remarked, that the whole mass of the blood is supposed to circulate once every three minutes throughout the body, and, therefore, if the action of the heart is virtually arrested for half this time, the ordinary relations between the circulating apparatus are disturbed. The arterial will have lost the greater part of its contents, and the capillary and the venous systems will have gained in this proportion. Both the loss and the gain are incompatible with the steady performance of the functions of life. This change in the distribution of the blood is inevitable. Did it not rest on undeniable facts, reasoning alone on the known properties of the series of vessels—on their rela-

tions to each other—on the large capacity of the veins—on the influence of the heart, would satisfy the most sceptical of the soundness of the opinion.

The blood is in motion at the moment that the heart begins to flutter in its action, and when this ceases it trickles forward in the direction of the capillaries and veins, and in these necessarily accumulates. The pulse, then, is no longer felt. On the return of sensibility, it is small, weak, and hesitating. During the suspense of the circulation, the chemical changes in the lungs are interrupted, and consequently the blood is less oxygenated than previously.

This inquiry into the modifications of the moving powers of the blood, under these circumstances, is essential, in order that a correct view may be formed of the nature of the objections to prevailing doctrines. The stoppage of hemorrhage on the occurrence of fainting is a fact, and, though contemporaneous with the weakened or interrupted action of the heart, and, in a general sense, consequent upon it, this does not prove that the capillaries are inoperative in the process of circulation, or that the heart is the sole moving power of the blood in the veins. Neither inference naturally flows from the phenomenon. Analysis will show that the physiologist has seized a general result, and has at once referred it to a remote and obvious primary cause, but has overleaped the consideration of intermediate agencies, which are links in the series of antecedents and consequents, associated in the production of a manifest effect—the stoppage of hemorrhage.

As the arteries, the respiration, and the alternate dilations of the right auricle, are admitted to play an important part in the process of circulation, with what strictness of philosophical reasoning can it be asserted, when these powers

are quiescent, that the cessation of hemorrhage proves the capillaries to be inoperative in the ordinary conditions of the animal system, or that the venous column is directly moved by the heart? The inference is not warranted by the phenomenon. All the powers which co-operate in the circulation are arrested; and how, in this case, the physiologist establishes the capillaries to be destitute of all influence, or the heart to be the sole cause of venous circulation, it is difficult to conceive. The capillaries and veins are in a state of distension. The former are not in a condition to contract upon their contents, and if they were, the large venous columns in advance of them, at rest, would be an obstacle which they could not overcome.

Other experiments have been instituted to determine the influence of the heart on capillary circulation, such as the excision of this organ—the passing a ligature round the aorta, or crushing the brain, the capillaries, at the same time being examined in a transparent membrane, placed in the field of of the microscope. The objection to all such experiments is the same as has already been offered to those of Magendie. It is not possible by such means to arrive at accurate results. If the heart be removed, or the aorta tied to intercept its action, the natural relations between different motory powers are at once destroyed. The arteries in this case cannot exercise any influence, nor can the blood be transmitted regularly to the capillaries, to enable the physiologist to study with advantage their functions. Were it admitted that capillary circulation did not continue long in any of these experiments, the inference that this was attributable to the non-co-operation of the heart would not be just. The arteries and capillaries cannot be expected to exercise their functions unless supplied with blood. A continued

current is furnished to neither. Were the blood, after excision of the heart, or tying of the aorta, accumulated in the larger arteries, it would be fair to ascribe such effect to the want of a propulsive power *a-tergo*. But what is the fact? Not that any accumulation takes place in these vessels, but indeed in the veins, proving that the blood after such operation traverses the capillaries.

Spallanzani, giving the results of his experiments, remarks: "Excision of the heart suspended the circulation in the whole system of the arterial vessels. It was different in the veins. The blood of the pulmonary vein and its ramifications continued its course for seventeen minutes; but it was much less rapid than formerly." Many experiments are detailed in corroboration of this fact. Müller, who considers the motion of the blood in the capillaries wholly dependent on the heart, acknowledges that "for a few minutes after separating the foot of a frog from the body, he could still perceive the motion of the blood from the minute vessels towards the larger, that is to say, towards the openings of the divided stems." "Baumgaertner observed the blood in the frog's foot continue in motion from three to five minutes, after ligature of the artery." Wilson Philip says, that "when a frog is decapitated without much loss of blood, and then a ligature thrown round all the vessels attached to the heart, on the web of one of the hind legs being brought before the microscope, the circulation in it is found to be vigorous, and will continue so many minutes, at length, gradually becoming more languid."

It is evident, after the preceding remarks, that the excision of the heart or tying the aorta, cannot possibly lead to any satisfactory results. To disturb or destroy any of the powers engaged in the circulation will necessarily

defeat every attempt to calculate the respective influences in operation. The experiments tend to prove that the capillaries exercise an independent propulsive power.

CHAPTER III.—THE PROPERTIES OF CAPILLARIES AS DETERMINED BY EXPERIMENT.

The larger arteries are admitted, by one class of physiologists, to possess elasticity and contractility. By another, the existence of the latter property is denied. It has, however, in my opinion, been placed beyond doubt by the experiments of Verschuier, Parry, Hastings, Thomson, Hunter, Bikker, Home, and others. The contractility of capillaries is shown by the application of different substances to delicate membranes, accelerating or retarding the circulation, according to the stimulating or sedative qualities of the agent.

These effects have been so frequently observed, and the conclusions which they establish are so strengthened by the greater or less facility with which different injected fluids pass through capillaries, as well as by the modifications which these undergo in disease, that it is difficult to imagine on what grounds physiologists deny them contractility. Müller, in a passage previously quoted, admits the continuance of circulation in the foot of a frog separated from the body, but refers it “to the tendency of the blood to escape from the divided vessels, which, by their elas-

ticity, assume a less diameter than they had before in a state of forcible extension. The narrowing of the vessels can, in fact, be perceived by the aid of the microscope. If the divided surface from which the blood flows is elevated, together with the leg of the frog, the escape of the blood ceases sooner, and after five or six minutes, not the slightest motion is perceptible in the capillary vessels."

Wedemeyer, who regards the capillaries as destitute of all motive power, nevertheless states that, on the application of galvanism "a remarkable retardation of the current of blood was produced, amounting in no long time to complete stoppage and coagulation." These effects, however, he refers not to a power of contraction in the capillaries, but to "the influence of galvanism on the nerves, and, through them, on the blood." To explain other phenomena, such as the circulation being carried on in some of the lower animals destitute of a heart, and in acardiac foetuses,—for the flushed appearance of the countenance in passion, and for the arteries being found empty after death, he maintains, "we must assume the existence of a vital attractive force in the tissues, a power by which the blood is drawn towards them, and which is regulated, as to its energy, by the nervous system."

Another authority remarks, "Weak volatile alkali or ammonia was applied in the same manner as spirits of wine and tincture of opium had been, and I had now the pleasure to observe a distinct, and in many instances, a complete contraction produced in the arteries to which it was more immediately applied. In upwards of one hundred experiments, in performing some of which I had the assistance of my friend Dr. Gordon, the contraction produced took place in less than two minutes after the application of the ammonia. In thirteen similar experiments

the contraction did not take place till after a period of three minutes. In three or four instances only, in which the ammonia was applied to the arteries of fresh frogs, were the contractions not induced; a proportion of failures surprisingly small, when contrasted with the number of times in which the appropriate mechanical and chemical stimuli are known to succeed, in exciting distinct contraction in the larger arteries of warm-blooded animals. The observations and experiments which I have just related, prove undeniably, I conceive, the existence of irritability in the smaller or capillary arteries of cold-blooded animals, and consequently, the possibility of irregular distribution of the blood in the particular parts of the body being produced, independently of the heart, by the vital, contractile, or irritable power inherent in even the minutest branches of the arterial system.”*

Hastings, in pursuing similar investigations, observes,—“it now only remains to point out the conclusions concerning the vital contractility of these vessels, which the facts obviously lead to. It appears that the application of a stimulus often quickens the circulation in the smaller vessels, whilst the motion of the blood in a neighbouring part, to which no stimulus is applied, remains unaffected.”† The study of these experiments can scarcely fail to convince the unprejudiced that the capillaries are endowed with contractility.

Another species of evidence corroborative of this opinion is derived from the greater or less facility with which different injections are made to flow through capillaries. Had they no power of contractility, or were they incapable of influencing the current conveyed to them, would it be

* Lectures on Inflammation, by Professor Thomson, page 83.

† A Treatise on Inflammation of the Mucous Membrane of the Lungs, page 59.

reasonable to anticipate that one class of fluids would be quickly arrested, whilst another was allowed an easy and uninterrupted course? Hales, in his interesting and ably conducted researches, shows that a column of warm water, pressing on the aorta, will flow freely through such vessels, whilst one of cold contracts them and passes with difficulty, requiring a longer period for its transmission. The same occurs with brandy and astringents generally. These experiments have been recently repeated, and with the same results, by Wedemeyer.

It may, perhaps, be urged that such experiments throw no light on the manner in which the capillaries act. This is at once granted. They establish, however, the presence and operation of contractility. The occasion which brings it into prominent notice is unnatural, but this furnishes no just argument against either its existence or its agency. It is, indeed, amusing to see with what ingenuity writers avoid the admission of such a power in contemplating the phenomena to which it gives rise. By one it is remarked, that "many substances, such as those called astringents, for example alum, seem to have the property of producing an approximation of the molecules of living animal matter,—and condensation of the matter,—and thus a contraction of the tissues which it forms. It is to this effect on the capillaries and small arteries that we must attribute the action of such astringents, and of cold, in arresting hemorrhage from wounds."* How peculiar is the language! Astringents have the property of producing *an approximation of living animal matter*, and all to avoid the simple term contractility.

It is contended, that, supposing the capillaries to possess such property, this cannot be shown to facilitate the motion

* Elements of Physiology, by J. Müller, M.D. translated from the German, by William Baly, M.D., Part i. page 226.

of blood, but, on the contrary, will be as likely to retard as to accelerate it. The objection arises from the consideration, that the contractions of the capillaries are not perceived to be successive, as those of the heart, by which globules of blood may be propelled.

Physiologists have repeatedly demonstrated that capillary circulation does not cease instantaneously on the removal of the heart, or the passing a ligature round the aorta. The vessels cannot act without the stimulus of blood. To intercept the supply, and then to point to the result as evidence that the heart is the only motive power, displays a limited acquaintance with the complicated functions of the sanguiferous system.

All such experiments are exceedingly defective ; nor is it possible to devise any free from insuperable objections, which cause either the destruction or the mutilation of important organs. The conditions induced by them are a wide departure from what is natural. But what force is there in the assertion that the capillaries, were they to contract, would be as likely to send the blood in a retrograde as in a progressive course? Behind, or on the side of the left ventricle, there is a *vis-a-tergo*, by which the blood is propelled into the arteries, giving immediate origin to the capillaries. Before, there is a gradually increasing capacity in the veins into which the capillaries pour their contents.

These circumstances are sufficient to prove that the contractions of the minute vessels will urge the fluid in one direction in preference to another. It must, also, be kept in mind, that the blood in capillaries is not observed to flow at times corresponding only with the systole of the left ventricle, but in a continuous stream, if that can be called a stream in which two or three globules a-breast jostle each other in their circuitous passages to the veins.

The motion of blood is slow and equable in the capillary and venous systems, and is admitted by those who argue that the circulation in both is wholly dependent on the heart.

It is astonishing with what stubbornness physiologists persist against any influence being assigned to the capillaries. The results of every experiment, apparently establishing the fact, are evaded by sophistry, or met by laboured objections. The following passage illustrates the justness of this remark :—

“Baumgaertner observed the blood in the frog's foot continue in motion from three to five minutes after ligature of the artery, and attributed the motion to a reciprocal action exerted between the nerves and blood; it most probably arose from the contraction of vessels which had previously been distended; anastomoses also might give rise to such appearances. The ingenious experiments of Baumgaertner unfortunately do not clearly prove what they are intended to do. Moreover, according to my observation, the circulation in the capillaries generally ceases very quickly on the compression of the artery of the limb, when the spontaneous motion of the red particles ought certainly to be seen, if it exists at all. Having destroyed the vitality of the heart of a frog by the application of *liquor kali caustici*, I could by means of the microscope, for some time perceive motion of the blood in the capillary vessels, but it depended probably on the compression of the blood by the elastic coat of the arteries which had previously been in a state of forcible distension.”*

The continuance of the circulation for some time after the ligature had been applied to the artery, is acknowledged by the writer, and certainly, the most natural mode to account for it is, to refer it to the ordinary contractile properties of

* Elements of Physiology, by J. Müller, M.D., Part i., page 223.

the capillaries. What other powers can possibly exercise any influence? He, indeed, remarks, "it most probably arose from the contraction of vessels which had previously been distended; anastomoses, also, might give rise to such appearances."

The distension of the vessels is assumed, which is not likely either to precede or accompany the experiment. The artery only being tied, the capillaries would cease to receive blood, but they would gradually convey what they possessed into the veins, and hence such condition would not be a very probable occurrence.

The congestion of vessels is always unfavourable to their action. It retards and never accelerates the motion of blood. It oppresses rather than invigorates the functions of the capillaries.

According to the reasoning of Müller, to enable these vessels to act, it is only necessary to produce distension; and then the motion of the blood will continue for a considerable period independently of the influence of the heart. If the capillaries can maintain the circulation for five minutes, when not at all influenced by any force or supply *a-tergo*, the fact undeniably establishes their contractility. If not endowed with this, no occasion would bring it into operation. The occasion in this, and in all similar circumstances, simply excites existing powers.

It is absurd to argue, that the capillaries, in a state of distension, can carry on the circulation, and, at the same time, to deny that they are endowed with contractility, or exercise an important part in the motion of blood. Even, agreeably to the views of this physiologist, the distension is only the occasion which brings into play the natural properties of the vessels. He imagines the effect to be produced by elasticity, but this cannot maintain the circulation. The

force which elasticity impresses upon a fluid, is derived from, and proportionate to, the cause by which the vessel is distended. Continued motion from elasticity alone is an impossibility. In the case under consideration, this property would not produce a series of contractions. According to the most liberal interpretation of its influence, it would cease to act the moment the distension was relieved, and how could this continue, when the ligature interrupted the impulse of the left ventricle? Such is the value of the objection enforced by a distinguished authority against the action of capillaries. In the first place, he assumes this condition of the vessels, and in the next, without explaining how it can produce the result, reasons upon it as a clear and admitted fact.

Anastomoses, it is urged, might possibly give rise to the appearances of capillary circulation. If arteries not included in the ligature afforded both an impulse and a supply of blood to the capillaries, which is the inference, what should prevent the longer continuance of the circulation? In his own experiment, no such cause could exist, for the vitality of the heart was destroyed, and yet the circulation was not interrupted in these vessels. Here, then, is a fact,—the continuance of capillary circulation, and the crude hypothesis of the physiologist, and one inadequate to the explanation of the phenomena.

It is stated that, if the circulation be impeded, “a pulsatory movement at each systole of the left ventricle becomes very manifest in the minute vessels.” This may be shown to originate in causes which have disturbed the relations of the several powers engaged in the circulation. An impediment to the free passage of venous blood invariably produces congestion of the capillaries; and such impediment is created in all experiments, which mutilate or cramp

the organ examined, or which torture the animal, either from the means employed, or the constrained position in which it is placed. In this case a continuous column is formed, extending from the obstacle to the left ventricle, the contractions of which communicate an impulse throughout the several series of vessels. The pulsatory movement in the capillaries and veins, synchronous with these contractions, is observed only under such circumstances, and is indisputable evidence that the natural relations of the vessels to each other and to the heart are interrupted; and hence the phenomenon affords no illustration of the influence of this organ under ordinary circumstances.

A writer of considerable talent and enterprize, and who does not allow that the capillaries facilitate the motion of blood, confirms from actual observation the truth of these remarks; and his own experiments may be adduced as exhibiting to a painful extent, a wide departure from just principles of investigation. He observes :—

“If the circulation be, on the other hand, in the slightest degree impeded, the pulsatory movement, at each systole of the heart, becomes very manifest. It is seen in all the three systems of vessels, arterial, capillary, and venous. In the arteries there is generally an alternate more and less rapid flow of the globules, at each systole and diastole of the heart; in the capillaries and veins, the blood is often completely arrested during the diastole, and again propelled by a pulsatory movement during the systole of that organ. In other cases, a pulsatory movement is seen in the arteries only, the blood in the capillaries and veins moving slowly but equably.

“The experiment is made in the easiest manner. A ligature being applied in the gentlest way round the limb of the frog, the circulation is immediately impeded in the

web, so as to render the phenomena which I have described quite apparent. The pulsatory, and the retarded or arrested movements of the globules of blood may be varied, in an infinite manner, by diminishing or increasing the degree of tightness of the ligature."

From this and similar experiments he concludes:

"It may now be asked, what presumption is there of muscular contractile power or irritability in the true capillaries?

"The flow of blood through the capillaries appears, in every instance, to be effected and modified by powers impressed upon it, of a character extraneous to any action of these vessels themselves.

"The influence of the contraction of the heart is quite obvious upon the motion of the blood in the capillary vessels, as I have already stated.

"Not less is the various influence of the struggles of the animal in retarding or arresting the flow of blood in these vessels, or rendering it retrograde.

"Any cause of contraction in the membranes forming the web itself has also a remarkable influence upon the course of the blood in the veins and capillaries.

"In vain we seek for evidence of a power to contract and propel the blood in any of these phenomena of the true capillary vessels. Where, then, is such evidence to be found?"*

The experiment is to determine how far the action of the heart extends, or, in fact, what powers are engaged in the circulation, and the first step taken, is to apply a ligature to the limb of a frog, which at once interrupts the passage of the blood flowing in the direction of the web,

* Critical and Experimental Essay on the Circulation of the Blood, by Marshall Hall, M.D.F.R.S.E., p. 75, 76, 85.

and prevents equally the return of it from the veins. The procedure clearly annihilates the influence of the contractility of arteries, and of every agent, whether it act behind or in advance of the venous column.

It is remarkable that the physiologist should not have been sensible of the unnatural conditions which the experiment produces; because, according to his views, the “arteries are, indeed, a second heart, in an elongated form. Their function appears to be so perfectly performed in health, that all visible pulsation from their action is lost at their extreme branches. The blood is carried along at last, by such gentle undulations, caught in their full force from the heart, but softened first by the elasticity, and then by the contraction of successive portions of the arteries, that its flow seems to become uniform.”* When the ligature is passed round the limb of the frog, the arteries cannot execute this function, receiving no blood. Having cut off this supply,—all-important, on his own views, and prevented the contents of the veins from escaping, he then examines the phenomena of capillary circulation. He observes that the globules seem to forget to march directly forward, indeed, in their astonishment, they move in all directions, and occasionally are at rest, as if hesitating what course to take. On one side, they have lost the assistance of the heart and arteries, and the aid of fresh streams of blood to stimulate them; on the other, the veins are incapable of transmitting forward what the capillaries have conveyed. The situation of the globules is one of great peculiarity, and yet the experimentalist, altogether insensible of it, reasons upon the phenomena, as if nature had allowed him to analyze her operations, when all her delicate and mysterious influences were at work.

* Opus cit. page 85.

It is not possible to imagine an experiment that deviates, in its conception and execution, more widely from the just principles of philosophy. Where are the globules to march to, when an obstacle is interposed to prevent their escape? Or how is the action of the capillaries to be determined, when deprived of the means essential to excite and maintain it? He remarks, that "the pulsatory and the retarded, or arrested, movements of the globules of blood may be varied, in an infinite manner, by diminishing or increasing the degree of tightness of the ligature." Unquestionably. To relax or tighten the ligature, is to affect the current of blood transmitted to the capillaries, and of course to change, with every such modification, the phenomena of capillary circulation. But with what propriety can these phenomena be brought forward as evidence of the natural influence of the heart, of the arteries, and of the capillaries, conjointly, or of each singly, on the motion of the blood?

The same remarks apply to all such experiments, whether the brain be crushed, the spinal cord destroyed, the heart removed, the aorta tied, or the animal tortured by the procedure adopted. In all these cases the natural relations uniting in harmonious action the several powers, maintaining and regulating the circulation, are destroyed.

In further illustration of the serious objections to researches by which organs are mutilated, and conditions induced widely different from those which are natural to the system, I shall introduce one other experiment from the same authority.

"A ligature was applied round the aorta of a frog. The circulation in the web, which was previously very vigorous, was almost immediately arrested, first in the capillaries, then in the veins. In the arteries there was a singular oscillatory movement of the blood for ten or fifteen minutes.

The globules of blood proceeded slowly onward for some seconds; there was, then, all at once, a rapid retrograde movement of the blood, apparently through the same space. This oscillation was repeated; the globules of blood were again moved alternately in progressive and retrograde directions as before.

“It appeared to me that the artery gradually contracted, in successive portions, and slowly emptied itself by propelling the blood in a continued stream along its final branches; that it then dilated suddenly, and drew the globules of blood in a rapid retrograde course.”*

In the consideration of this experiment, he remarks:—

After the power of the heart is thus excluded, what do we observe? A peculiar, slow oscillatory movement of the blood in the arteries, whilst that in the capillaries and veins is motionless.

“If the capillaries and veins have the same power of irritability as the arteries, why do we not observe the same phenomenon in them as in the arteries?”

“If the capillaries possess a muscular power, why is the blood all at once motionless in them, when the power of the heart is excluded? Must it not rather be inferred, that, as the blood contained in these vessels is without motion, the vessels themselves are without automatic power?”

The changes in the condition of the circulating powers are the same in character, whether a ligature be passed round the limb of the frog, the heart be removed, or the aorta tied. The experiments differ only in the amount of general disturbance induced. The reasoning which applies to the one is strictly applicable to the other. When the ligature is passed round the limb, the parts below it

* *Opus cit.*; p. 79.

neither receive blood nor expel what they possess. When the heart is removed, there is at once an interruption to the supply. The circuit is broken, by which alone the supply and onward movement of the fluid can be regularly maintained. When the aorta is tied, the circuit is broken, as effectually as if the heart were removed. In this case, the stream essential to the action of the arteries and the capillaries is cut off, and the veins cannot possibly convey forward the fluid transmitted to them, for to tie the aorta is to place a barrier to its escape through the heart.

In the experiment in question, the application of the ligature, as already remarked, would force the blood in the direction of the capillaries, independently of the influence of the aorta; and being in motion, antecedently to the application of the ligature, it is manifest that the blood at this moment is pursuing its course, in consequence of the impulses previously imparted to it by the heart,—the sudden compression of the aorta by the ligature, and, perhaps, aided by the contractile properties of the artery. The agency of the two first causes is indisputable, and if to these be added, the contractility of the vessel, the subject becomes exceedingly complicated and difficult. To determine how much of the effect—the forward movement of the blood, depends on each, and the discrimination must be made, or the reasoning will rest on assumptions, is impossible in the existing state of physiology.

The gradually diminishing capacity of the artery, or its seeming contractility during the experiment, is no evidence that the contents are expelled by its action. The calibre lessens with the escape of the blood, but the escape is no proof of the existence of contractility. The effect cannot be thus simply traced to its cause, and yet such is the view of the writer and of physiologists generally.

It is stated in explanation of the experiment, that the artery propelled the blood by its contraction ; and subsequently by its dilatation, “ drew the globules of the blood in a rapid retrograde course.” The blood will flow where it meets with the least resistance, but it must already be in motion, or influenced by some circumstance independently of the dilatation of the vessel, to cause it to take one direction in preference to another. The dilatation will not create the motion. The experiment in no degree elucidates the phenomena of circulation. It throws no light upon the action of the capillaries. It cuts off the stream from behind, and places a column of venous blood, in advance of them, at rest.

The conception of the experiment and the arguments with which it is accompanied, show how little attention the relations of the moving forces of the blood have received from minds of a superior order, familiar with scientific investigations, and rich in the occasions necessary to conduct them with success.

In further illustration of the importance of maintaining, in experimental researches, the natural relations of the moving powers of the blood ; and of the little consideration which the subject has received from physiologists, I shall briefly allude to a difference in the condition of capillary circulation, when the blood is enabled to escape from divided vessels ; and when the membrane examined is exposed to the rays of the sun.

In regard to both circumstances, it is remarked, “ There are two conditions under which the blood in the capillaries of a transparent part separated from the body may still be seen in motion by means of the microscope. 1. As long as the blood continues to flow from the divided vessels. Thus, for ten minutes after separating the foot of a frog

from the body, I could still perceive motion in the blood from the minute vessels towards the larger ; that is to say, towards the openings of the divided stems. These movements, in my opinion, depend simply on the escape of the blood from the divided vessels, *which, by their elasticity, contract to a less diameter than they had before while in the state of forcible distension.* 2. The condition under which the motions are perceptible is, when the rays of the sun are allowed to fall on a moist part separated from the body. The surface of the part then becomes dry and wrinkled so quickly, that the change is perceptible to the eye.”* In the analysis of the experiments on capillary circulation, two objections have been particularly dwelt upon, viz. the stoppage of the current from behind, and the interruption to the escape of the contents of the capillaries ; and both effects are produced when the heart is removed, the aorta tied, or a ligature passed round the limb. The procedure, in which the vessels are divided, obviates one of the objections. The capillaries cannot receive a current to excite them, but they are enabled by the divided vessels to urge forward the blood, and all authorities agree, that at this time circulation is carried on with considerable vigour.

The physiologist ascribes the movements to “the escape of the blood from the divided vessels, which, by their elasticity, contract to a less diameter than they had before, while in the state of forcible distension.” From his own views strange conclusions may be drawn. The escape of blood is alleged as the reason for its motion, elasticity diminishing the diameter of the vessels. Therefore, in experiments in which the vessels are not divided to allow of such escape, by which either elasticity or contractility

* Müller, Opus cit. Part. i. page 223.

may be enabled to operate, with what philosophical propriety can the physiologist argue that these vessels are incapable of propelling the blood? He admits the power, when the occasion is favourable for their action,—no obstacles existing in advance of them; but denies it, when the vital fluid by which they are stimulated has no means of escape.

If the divided vessels cause circulation simply by relieving distension, it would be arrested the moment the capillaries had passed onward a portion of their contents. It is observed, however, for ten minutes, during which time they receive no streamlets from behind, to maintain continued contraction.

Again, the circulation is excited when the direct rays of the sun fall upon the vessels. The same effect occurs when various stimulating agents are applied, and the heat of the sun acts in a similar manner.

The justness of these remarks on the imperfections of the various experiments, instituted to elucidate the functions of the capillaries, is fully established by the difference in the results occasioned by divided vessels. The analysis has constantly presented to the mind two objections,—an obstacle in advance of the capillaries, and an interruption to the supply from behind. In the proceduro in which vessels are divided, one of these objections is shown to be removed; and hence the continuance of circulation, as long as the capillaries can be supposed to possess any fluid to excite them.

Sensible that all such experiments are fraught with insuperable difficulties, and throw no light on the functions of the capillaries, the consideration has been to devise some method by which the fact of their operation might be clearly established—an experiment, indeed, in which

the properties and relations of the vessels examined should not be seriously affected, nor the appreciation of the phenomena require the aid of a powerful lens. An organ suggested itself, as peculiarly adapted to the inquiry, viz. the placenta. It is composed chiefly of arteries, veins, and capillaries, and although the disengagement of it from the uterus disturbs the normal relations of these vessels, it appeared, nevertheless, applicable to the subject of investigation. Nor was it necessary, as a preliminary step, to determine the nature of the connexion between the placenta and the uterus. Whether direct or indirect, is of no importance to the present experiment.

The umbilical vein carries arterial blood from the placenta to the fœtus,—the umbilical arteries convey venous blood from the fœtus to this organ. The origin and termination of these two classes of vessels in the placenta are involved in much obscurity. No direct connection is traced between them. Whatever opinions may be held respecting the functions of this organ, or its relation to the uterus, it will scarcely be doubted that the vein terminates in capillaries, and that the arteries originate in the same kind of vessels. It is not my intention to examine the phenomena of fœtal circulation, but to allude only to one striking peculiarity, viz. the circulation of blood in the umbilical vein. This fluid is transmitted from the placenta to the fœtus without the aid of any propulsive organ. The capillaries are, indeed, the only source of motive power shown to exist, and hence, the placenta, separated from the uterus, appeared capable of determining the influence of capillaries, in urging the blood through the long capacious vein. To test the fact, a placenta was procured, twenty minutes after separation from the uterus, and placed with the exception of the cord, in a bladder, which was immersed

in water, at the temperature of 100° Fahrenheit. The free extremity of the cord, at the same moment, was elevated to an angle of 30°, resting on the edge of a glass, and at the distance of a foot from the placenta. At the commencement no blood escaped from the vein, but in two minutes from the immersion, it began to flow, and continued for about twenty minutes, and at this time, the glass had received above an ounce.

Here, then, is an experiment, unexceptionable in its character, demonstrating the power of the capillaries to carry on the circulation, not only in their own complicated net-work of vessels, but in larger vessels, ultimately terminating in a capacious vein. The difficulty to the motion of the blood was intentionally increased, by the elevation of the whole cord above the level of the placenta. Had this organ been immersed, without the bladder, the absorption or imbibition of the water would have invalidated the results. The water is employed for the purpose of maintaining, what may be conceived to be, the natural temperature of the placenta.

The ascent of the blood arises entirely from the influence of the capillaries. The water excites them to contraction, and the escape of the blood is not opposed by any impediment. The experiment produces no important modification in the conditions of this fluid. The water is not absorbed, nor is the temperature of it elevated above the heat of the body. The consideration of the circulation in this case is not complicated by circumstances acting *a-tergo*, or in advance of the blood; nor by the agency of respiration, or the struggles of an animal in torture, or placed in a constrained position. Whether the results be regarded as satisfactory or not, the experiment is manifestly free from many objections, which apply to the labours of others in the same field of physiological research.

As all vital actions are effected in capillaries, it is natural to suppose that these will be endowed with properties capable of influencing the motion of the blood transmitted to them. The frequent normal changes in the activity of particular organs, originate in these vessels, either directly or through the instrumentality of the nerves ; and the modifications induced in the larger arteries, are the consequences springing out of these local changes. When the capillaries are excited to increased action, the blood flows to them in strict correspondence with the demand : when the action is diminished, the supply is proportionately weakened. It is difficult to imagine on what grounds writers deny to these vessels an important influence in the process of circulation.

In support of the view which is here advocated, a distinguished authority, remarks : “ It appears, therefore, that we are fully warranted in the conclusion that the arteries possess a proper contractile power, and it is to be presumed that this power resides in their transverse fibres. It appears likewise to be established, that this contractile power is principally seated in the capillary arteries, while the large trunks and the veins, although not destitute of it, possess it in less degree. Indeed, every fact, with which we are acquainted respecting the mechanism and functions of the sanguiferous system, lead us to the same conclusion, that the large arteries are to be regarded as canals transmitting the blood from the heart, where it receives its great impulse, into the smaller branches ; and that it is principally in these smaller branches that it exercises its various functions. We are, therefore, to consider the large trunks in the light of a mechanical or hydraulic system, and the capillaries as physiological or vital organs.”*

* An Elementary System of Physiology, by John Bostock, M.D., F.R.S., third edition, p. 244.

BOOK V.

ON THE CONDITION OF THE BLOOD IN THE VEINS, IN THE NATURAL AND THE DISTURBED STATES OF THE ANIMAL SYSTEM.

CHAPTER I.—PROPERTIES OF THE VEINS AND THE PECULIARITIES OF THE VENOUS SYSTEM.

The forces employed in the circulation of the blood cannot be examined altogether independently of each other. They are so intimately connected, co-operating in the production of the same general effect, that the whole will frequently be brought, more or less, under review. Previously to the consideration of the powers by which the blood is moved in the veins, it is important to have clear ideas concerning the origin of these vessels, otherwise the evidence of the forces acting *a-tergo* will necessarily be defective.

Haller supposes the origin of the veins as numerous as the terminations of the arteries, and the same view has also been generally entertained by subsequent writers. According to Bichat, the veins arise from the general

capillary system. Magendie has shown that the venous radicles are continuous with the arteries and lymphatics ; and the discovery of a communication between the lymphatics of the intestines and the mesenteric veins,* in some animals is in favour of this opinion. With this exception, the venous radicles are generally considered to originate in the arterial capillaries, and the connection between the two is regarded as direct. That such connection exists is indisputable ; but there may be other channels of communication with the venous system, which the elaborate investigations of physiologists have not yet discovered.

The termination of lymphatics in veins is an important fact in relation to the present inquiry. It proves that fluids flow into these vessels independently of the action of the heart ; and were the same established with respect to other minute vessels, the prevailing views on the circulation would necessarily be greatly modified. According to these, the blood is urged through the capillaries into the veins by the impulse of the left ventricle. The capillaries examined by the microscope present phenomena which can scarcely be brought forward in confirmation of this doctrine, though they are usually regarded as furnishing decisive evidence in favour of it. The following description of capillary circulation is given by Magendie, the accuracy of which will not be questioned :—

“Your eye has followed the progress of the globules suspended in a transparent fluid ; you have seen them roll over each other, and jostle each other, sometimes advancing singly, sometimes presenting several abreast, without the circulation appearing to be for a moment obstructed or suspended. What I most desired to make perfectly obvious to your sight was the place of motionless repose close to the

* The Researches of Dr. Fohman.

parietes of the vessels, our knowledge of which is due to M. Poiseuille. None of you can now call its existence in question; the sluggish motion of the globules which approach it, and the total stoppage of those which are plunged in it prove abundantly *that the whole of the sanguineous column does not take part in the movement.* Besides, is it not evident that there is an interval between the circumference of the cylinder and the part occupied by the current? This interval is necessarily filled by a liquid, and that liquid, from its transparency, can be nothing but the serum. The capillaries of a certain diameter were the most suitable for this observation; for, having arrived at an extreme degree of tenuity, there was nothing to be seen, but a small thread of fluid, which could detach them from the place of motionless repose. On the contrary, when the vessel presented some volume, nothing was easier than to verify the different degrees of rapidity of each globule. In the centre there was a rapid propulsion; more out of it, a slackening of motion; close to the parietes, a complete stoppage.

“The progression of the column was by no means always regular. We saw it, from time to time, stop, briskly resume its course, project itself towards a collateral branch, to the right, to the left, and flow back without the physical conditions of the canal appearing to have been in the least possible degree modified. We have briefly recalled to your recollection the explanation of these phenomena, on which we have already dwelt enough. I shall not return to the subject.”*

Does this picture of capillary circulation establish the pervading influence of the left ventricle? In this case, the

* Leçons sur les Phénomènes Physiques, Tome iii. 1837, pp. 343, 344.

contents of the capillaries would be urged simultaneously forward. The small columns of blood are not, however, marked by a regular progressive motion. At one moment they are suddenly arrested ; at the next, they resume their course towards a collateral branch, to the right or left ; or flow in a retrograde direction. The globules do not exhibit the same condition in any single point of the capillaries. When contiguous to the sides of the vessel, they are nearly at rest ; when approaching the centre, they move with accelerated speed.

The pressure which is imagined to be transmitted to, and through the capillaries, does not, according to this account, affect their contents equally. The irregular movement of the globules is not explicable on this supposition. It must be referred either to their own vital properties, or to the action of the vessels. The description, it is important to bear in mind, is derived from an examination, not of the exceedingly minute, but of the larger capillaries ; consequently it remains yet to be shown in what manner blood circulates in the former, which from their extreme tenuity elude accurate observation.

It is the opinion of some that the communication between arteries and veins is not maintained by vessels, but by channels destitute of membranous parietes. The dispute concerning the nature of the connexion shows the difficulty of the question, and the perplexity in which it is involved. The influence of the heart on venous circulation has been inferred by the easy transmission of fluids from arteries into veins. Open channels existing between the two, the pressure of a column of blood in the one, is stated to account for the rise of blood in the other to an equal height. The tendency of fluids to establish an equilibrium is supposed to place the fact beyond all cavil. The hydrostatic prin-

eiple does not strictly apply to the phenomena of circulation. It will at once be admitted that fluids may be made to pass from arteries into veins, but this is no evidence that, in the natural state of the vital powers, the blood flows into the latter, either from the direct contraction of the left ventricle, or the pressure of the arterial column. This is assumed, and not proved.

In the preceding investigations, it has been shown that the capillaries exercise an independent action, in virtue of which the blood they receive is conveyed into the veins. If such power exist between the arterial and venous systems, with what force of argument can it be laid down, that the blood in the latter ascends from a tendency which fluids have to find their level? Were the connexion between the two systems maintained by inert tubes, the reasoning would be perfectly just, but only on this supposition. Before entering on the discussion of the subject, it may be well to offer a few general remarks on the venous system, the striking conditions of which are thus accurately described by Bichat.

“1. General pulsation in the arteries; absence of this general pulsation in the veins. 2. Rapidity of the current of blood in the arteries: sluggishness of the same current in the veins. 3. Greater capacity and thinner parietes of the veins; smaller capacity and greater thickness of parietes of the arteries. 4. Necessity of accessory aids in the venous circulation; absence of such necessity in the arterial. 5. The jet of blood in the latter being effected *per saltum*, in the former being uniform. 6. The liability of blood in the veins to be influenced by gravitation and other adventitious causes; the absence of all such influence in the arterial movement. We have here a series of phenomena which, after what has been said, must evidently depend on the existence of an impelling agent at the origin

of the arteries, and the absence of such an agent as that of the veins."*

The parietes of the veins are much thinner than those of the arteries, and in common with them are enveloped in a loose filamentous tissue derived from the organs to which they belong. Their middle or proper venous tunic is composed of fibres, having almost exclusively a longitudinal direction. They are softer and more extensible than those of arteries; their internal membrane is smooth and highly polished. It is much thinner than the corresponding arterial membrane, but is much more distensible and less fragile. This membrane is the most extensive and uniform in its distribution of all the venous tissues; it is, indeed, the only one found in the substance of organs, as in the liver, kidneys, spleen, and brain. In certain veins, it presents folds which are named valves, the office of which is to prevent the retrograde motion of blood. The veins are much more dilatable in the transverse direction than the arteries, but less longitudinally. The difference in the circulation of blood in the two classes of vessels requires this modification. In many slight, as well as serious disorders of the vital powers, congestion frequently occurs in the veins, but never in the arteries alone. In these the blood readily flows forward, impelled by the contraction of the left ventricle; but in the former the circulation is often impeded, and occasionally the blood accumulates in them to an extraordinary degree.

The extensibility of the veins may be shown to exercise an important function in the animal economy. These vessels being enabled to receive and retain an inordinate quantity of fluid, the heart, lungs, and other viscera are

* *Anatomie Générale précédée des Recherches physiologiques sur la vie et la mort*; Tome premier, p. 387.

necessarily protected from frequent causes of derangement. Were the blood, which is thus accumulated, distributed to any of these organs, injurious and perhaps fatal effects would be induced. This function of the veins is susceptible of copious illustration, and the knowledge of it is of great practical value.

The contractility of the veins is admitted by most physiologists, and is stated to be much greater in a lateral than a longitudinal direction. This property is imagined to be proved by various experiments. When a ligature is passed round a vein, the blood, on the side of the heart, flows forward. When two ligatures are applied, the blood, in the included portion, is ejected on the vein being punctured. The venous current, in escaping, is considered to be modified by the contractility of the vessel. The influence of this power is supposed to be observed in a peculiarly marked degree in the superficial veins. In summer they are dilated—in winter contracted. The immersion of any part of the body in warm or cold water presents similar phenomena. These facts are, however, by no means so unexceptionable and conclusive as is imagined,

The contractility of the veins is altogether denied by some writers. One who has given much attention to this subject remarks, that “the veins are neither irritable nor elastic; they are very dilatable, but have no reaction.”*

The blood beyond the ligature flowing towards the heart is certainly no proof of the exercise of contractility. Before the application of the ligature it was moving in this direction, at least, in virtue of one power, the *vis-a-tergo*, and perhaps facilitated in its course by changes in the capacity of the chest. The ligature cannot possibly subtract the motion previously imparted, and if any portion of it is

* Dr. Carson.

derived from causes acting in advance of the stream, the means employed are clearly incapable of elucidating the question

The application of the ligature may be shown to urge the blood forward. Were the vein removed from the body and filled with water, this would readily escape from any aperture, left open, on passing a ligature round the vessel. The experiment cannot possibly be advanced to prove that venous circulation continues from the contractility of the veins. The ejection of blood from the portion included between two ligatures, admits of explanation widely opposed to the received doctrine. The ligatures cannot be applied so that the extent of vein included shall contain the ordinary quantity of blood. If the ligature on the side of the heart be tied last, the quantity will be less than natural; if first, greater. But supposing both to be simultaneously adjusted, to obviate this objection, the contents of the vein will inevitably be increased. The pain inflicted on the animal, in the preparation of the experiment, will render the breathing quick, constrained and laborious, and the invariable effect is distension of the venous system. Hence the vein included between the ligatures is not placed in its usual condition. The contents are augmented, and being suddenly arrested, the blood will clearly be ejected on puncturing the vessel, not from the contraction of the venous parietes, but from the tendency of it, the globules of which it is composed being in agitation, to flow where the resistance is the least. These circumstances have never been taken into consideration. Admitting contractility to be the cause of the phenomenon, the experiment does not, in any degree, elucidate the influence of it on the venous current in the undisturbed conditions of the system.

The jet of blood in venesection is asserted to be modified by the contractility of the punctured vessel. There is no

proof of the fact. The changes observed in it are attributable to the contraction of muscles in the vicinity of the vein, or to the altered character of the breathing. They may be produced at pleasure. Coughing or strong expirations will invigorate the stream. A continued series of deep inspirations, or almost suspended breathing, will arrest or enfeeble it. All muscular actions cause the blood to be ejected with increased force.

Contractility is further stated to be established by the manner in which the superficial veins are affected by cold and heat. In the one case they are constricted—in the other dilated. These effects are evident, and much less exceptionable than the preceding phenomena. They occur, indeed, in the natural condition of the vital powers, but are not altogether free from objections. Cold will constrict the arteries as well as the veins on the surface of the body. The influence exerted cannot possibly be partial. The former being depressed will carry a diminished quantity of blood to the latter, and hence the reduced contents of the veins cannot justly be referred to the contractility of their parietes. The phenomenon may arise mainly from the constriction of the capillary arteries. If these carry a stinted supply of blood to the surface, the veins, as a necessary consequence, will receive less.

The dilatation of the veins from external heat requires few words. They are admitted to possess extensibility in a remarkable degree, and it is apparent in all cases of congestion. They are also endowed with contractility, but to a very limited extent, nor can it be shown to accelerate the circulation of venous blood. This is slow and equable, exhibiting no pulsation, except occasionally in the vicinity of the heart, from the contraction of the right auricle.

The disproportion in the capacity of the arterial and venous systems is an interesting and important fact con-

needed with the present inquiry. Many of the early physiologists endeavoured to ascertain the precise difference; there is not, however, any strict agreement in their calculations, nor is it easy to arrive at accurate results. The means usually employed can make only a distant approximation to the truth. Injections distend the vessels, especially the veins, and consequently produce a greatly exaggerated capacity; but independently of this difficulty, the difference between the two systems is not constant and unvarying, but changes from birth to advanced age. In early life, the arteries have a greater proportional capacity than at any subsequent period. At this time they are exceedingly active, as is evident from numerous phenomena; and this energy of function is necessary to nourish and develop the body. It diminishes, however, with the progress of years, during which the veins gradually acquire an increased capacity.

The difference between the two systems is very striking in old age, particularly in weak and debilitated constitutions. At this period, not only the deep-seated, but even the superficial, veins are greatly enlarged. These changes are principally attributable to two causes:—diminished activity of the organs of circulation, and the narrowed sphere in which the blood is confined. The heart, the arteries, and capillaries possess little of the energy of early life, nor, indeed, are their actions roused by the various circumstances which exercise considerable influence at this season, such as the gay and buoyant feelings of the mind—the strong and frequent muscular contractions of the body, inseparable from the sports and pursuits of youth—abundant and well-digested food. These causes tend very powerfully to invigorate the motion of the blood, and to equalize, as far as the natural capacity of the two systems

will allow, its distribution. In the decline of life these secondary, nevertheless important, conditions exist but in a lessened degree. The circulation is also materially circumscribed. The capillaries, which in early life are exceedingly numerous, gradually become obliterated in the progress of years; the inevitable effect of which is to give to the veins, a greater proportional quantity of blood.

It is worthy of remark, that in all cases of debility, whether in consequence of age, temporary or permanent disease, the blood always accumulates in these vessels. They are a reservoir from which the arteries derive a supply on the returning symptoms of health. Without such provision recovery would frequently be impossible. The improvement gradually re-establishes the balance of circulation, invigorating the animal system before there is much nutritive matter received from without. These changes are not only interesting in a physiological point of view, but have the most intimate relations to inquiries concerning the nature and treatment of diseases.

It is evident that the two columns of blood,—the arterial and the venous, are greatly modified in the different stages of existence. The venous system always exceeds, and to a considerable extent, the capacity of the arterial; but much less from birth to maturity, than from this period to advanced age. When the heart and its associated organs of circulation are the least capable of propelling their contents, the column of blood to be moved in the veins is the greatest; and yet eminent authorities assert that it is directly influenced by the contractions of the left ventricle, and they are deemed the only efficient cause of motion. The difficulties with which this opinion is fraught have led others not only to doubt its soundness, but to propose a very different explanation.

The removal of pressure from the upper part of the *vena cava*, by the dilatation of the chest, has been regarded as necessary to account for the circulation of blood in the veins. If we consider that the venous column is augmented, and the *vis-a-tergo* diminished, with advancing years, and in the varying conditions of the body, the imagined extensive influence of the heart will be acknowledged to rest on questionable data.

Haller, in his elaborate work on physiology, presents numerous calculations of the capacity of the two systems. Estimates are given of the relation between arteries and their corresponding veins, and it is shown that the capacity of the latter always exceeds that of the former, and in some instances almost to a quadruple extent. The difference between the smaller arteries and veins is rather a matter of conjecture than experiment. It is, however, admitted by all writers, that the latter greatly surpass, both in number and capacity, the former. The difference between the two systems is represented by Haller, in the ratio of 4 to 9. This can be regarded, however, only as a distant approximation to the truth, and does not convey any idea of the temporary or permanent changes produced by disease, in which the difference will be much greater.

An inquiry into the powers which move the blood in veins would be imperfect, unless certain peculiarities belonging to the venous system were noticed. The consideration of them will show how improbable the doctrine is, that the force of the heart extends through the venous radicles to the right auricle. The manner in which circulation is carried on in the brain affords a strong objection. The blood sent to this organ is distributed throughout its substance, as in other parts of the body, the connection between the ultimate capillary arteries and veins

has, however, something singular in its character. The The latter, in place of pouring their contents into trunks, capable of maintaining a uniform pressure from the left to the right side of the heart, convey them into membranous reservoirs, termed sinuses, formed by the separation of the plates of the *dura mater*. These sinuses communicate with each other, and are the only channels uniting the proper cerebral veins with the jugular. It can scarcely be urged that the blood moves in these sinuses in virtue of the impulse of the left ventricle.

The arterial fluid meets with many obstacles in its flow to the brain. It circulates against its own gravity, and much of the force with which it is propelled is lost in striking against the angular curvature formed by the internal carotid in its passage through the petrous portion of the temporal bone. The blood which enters the cranium is very much retarded in its motion by the peculiarly situated, as well as the minute divisions of the arteries, previously to arriving at the cerebral substance. These conditions protect this delicate and important organ from shocks which would be felt were the blood sent to it in a copious and uninterrupted current. The contemplation of these conditions will scarcely allow the supposition, that the force of the heart, immensely weakened by the difficulties opposed to the transmission of the blood, is nevertheless sufficient to convey this through a mass of arterial and venous capillaries, and at length through large sinuses, which cannot aid the circulation, to the termination of the veins in the right auricle. The force adequate to the production of this effect is certainly not furnished by the heart.

A just explanation of the phenomenon is found in the powers possessed by the capillaries. The heart conveys

the blood through the arteries and their subdivisions, but when it reaches vessels of exceeding tenuity, it no longer circulates from a *vis-a-tergo*. The blood, after this complicated and devious circulation, will unquestionably flow into the jugular veins in a slow and equable stream. The direct influence of the heart cannot possibly be traced in them. A contrary opinion is, however, maintained by an ingenious writer, but on very slender grounds. The occasional pulsation of the jugular veins is attributed by him to this cause. "The arteries which convey the blood to the head throw this fluid into the cranium by synchronous jets. An equal quantity, according to the hypothesis contended for, must, at the same instant, be discharged from the cranium into the veins. This must also be by jets which cause successive currents, or, in other words, pulsation in these veins."* The blood in no part of the venous system exhibits the successive impulses of the heart. Its long and tedious passage through minute capillaries, divided into globules, circulating alone or in pairs, render it incapable of transmitting to the contents of large veins the successive contractions of the left ventricle, or of flowing into them from its numerous channels, in times exactly corresponding with these contractions. The globules can neither communicate the force originally impressed upon the arterial column, nor can they retain it, so that when collected the stream resulting moves in jets synchronous with those of the arteries. This effect cannot possibly be produced by the transmission of blood through capillaries, and it is less likely to occur in the jugular veins than in any vessel except the *vena cava hepatica*.

The imagined necessity of a quantity of blood escaping from the cranium, precisely equal to that which enters, is no corroboration whatever of the opinion of this physio-

* Dr. Carson.

logist. When the vital organs become diseased, the strict relation between the two quantities is interrupted, and yet the blood continues to circulate. The phenomenon is observed, and in a very marked degree, whenever, from disease of the lungs or heart, the blood is prevented from flowing freely into the pulmonary artery. The pulsation arises from the retrograde motion of the venous fluid, and not from the successive impulses of the arterial. It invariably exists in the circumstances mentioned.

Another remarkable peculiarity in the venous system presents itself in the liver. The blood which has been sent to the abdominal viscera passes, after performing its respective functions, into venous radicles, and ultimately into capacious veins. These, however, do not convey it to the heart, but terminate in one large vessel, the *vena portarum*, which enters the liver, and divides and subdivides into minute branches after the manner of an artery. The blood is again collected by venous radicles, which unite, forming gradually converging trunks, and at length one capacious vessel—the *vena cava hepatica*. Hence, in this instance, the blood, before it arrives at the heart, has circulated through four sets of capillaries, one arterial and three venous. Is the impulse of the heart adequate to this complicated circulation? To suppose it capable of urging the blood through this extensive and devious course, without deriving any assistance from the capillaries, is ascribing to it an extraordinary power. In previous investigations the influence of respiration—of the heart—arteries and capillaries, on the motion of blood has been studied, and in pursuing the present inquiry, a reference to results and experiments which have already been brought under consideration is unavoidable—the same experiments being adduced by physiologists in confirmation of their views with respect to arterial, venous, and capillary circulation.

CHAPTER II.—EXAMINATION OF EXPERIMENTS SUPPOSED TO ESTABLISH THE DIRECT INFLUENCE OF THE HEART ON THE MOTION OF VENOUS BLOOD.

Venous circulation has always been regarded by physiologists as an exceedingly difficult and abstruse question. The length of column to be moved, its distance from the propelling power, and the intervention of minute capillaries, are circumstances well calculated to give rise to various speculations. The column has not only these peculiar conditions, but is almost three times greater than the arterial, which is imagined to urge it forward. Physiologists have endeavoured to discover forces adequate to the transmission of the blood through its long and complicated circle. Magendie finds them in the action of the heart and arteries. Barry, in addition to these, in the diminution of pressure in the chest on inspiration. Others regard the heart, the arteries, and the capillaries, conjointly, as the efficient causes. It is laid down by Arnott, that the weight of a column of blood, in any descending artery, is capable of raising a mass to an equal height in the corresponding vein.

The experiments of Magendie have been shown to be fraught with serious defects. They do not establish the

direct and uninterrupted influence of the heart on the venous column. According to his own admissions, blood flowed from the punctured vein some time after the *vis-a-tergo* was wholly arrested, and, indeed, only ceased when there was no blood in the capillaries and artery to afford a continued supply. In his researches to determine the influence of the heart and arteries, the whole of the sanguiferous system was disturbed, and consequently no just inferences respecting these could be deduced from them. His numerous and elaborate inquiries into venous circulation, are indeed open to formidable objections.

Errors in physiology are to be traced to two very different classes of writers. The one, in the study of the animal machine, does not sufficiently value the light of physical science reflected on his investigations. The other will not acknowledge the peculiarity of vital phenomena, and their modifying influence in all the important processes of life ; but is constantly endeavouring to reduce them to the familiar laws of inorganic matter. Both tendencies are unquestionably injurious.

According to Arnott, the simple weight of a column of blood, in any descending artery, will raise the contents of a corresponding vein to an equal height. This would certainly be the case, were the vessels connecting the two open and presenting no obstacles. The capillaries are intermediate, of which little is known. The description which is given of capillary circulation by various writers would scarcely lead us to suppose, that the motion of blood in them depends wholly on a *vis a-tergo*. No continued or regular progressive stream is observed extending from the artery to the vein. The connection between them is maintained by globules, alone, in pairs, or sometimes three a-breast, which at one time are at rest ; at another urged

forward with a momentary accelerated force; and then, perhaps, return upon their previous steps. Such is the picture of capillary circulation, furnished by the microscope in the hands of Magendie. These globules are regarded as the carriers of the impulse of the heart, and yet their motion is marked by the greatest possible irregularity. If these exhibit such phenomena, in what condition are those in still smaller vessels on which the microscope throws no light, being, indeed, too minute to admit of accurate examination, and yet the important operations of life are chiefly effected in vessels of this character?

The blood, which is one moment in the aorta, is not immediately conveyed into the *vena cava*. The properties of the fluid contained in each are widely different; and where the change takes place, or what is the nature of it, is involved in doubt. The blood both loses and acquires something at every revolution. The loss, as well as the gain, is the consequence of vital action. The interchange of globules occurs perhaps in the exceedingly minute capillaries, to which the influence of the heart cannot extend. Physical science presents no objects analogous to these vessels, nor can it determine either their nature or functions.

The capillaries have frequently been examined; the animal, however, selected for experiment, has either been tortured, or the organ placed in the field of the microscope has been so constrained or injured by its position, or other causes, that the phenomena observed could be no just indications of capillary circulation in a normal state. In illustration of the truth of this remark, attention is directed to the following experiment:—

“The crural artery and vein of a frog being insulated by careful dissection, after a tight ligature has been passed

round the leg, the circulation in the foot is continued only by these vessels. The animal is pinned on a layer of cork, so that the interdigital spaces shall suit the object-glass of the microscope. The progress of the blood in the capillaries is then examined. When the degree of rapidity of the globules is ascertained, the course of the blood in the artery is interrupted, while the vein is left free. The globules still continue to move, but more slowly. The motion becomes slower and slower, and entirely ceases at the end of two or three minutes. Let the pressure be withdrawn, and instantly each globule which was in a state of complete repose shoots forth like an arrow, and resumes its normal rapidity.”*

This experiment may be adduced with much greater force to prove that capillary circulation is independent of the heart than the contrary, which it is imagined to establish. The web of the foot has no connection with the general sanguiferous system, except by means of two vessels, an artery and a vein, and therefore, by compressing either, it is easy to influence the circulation in it. The experiment is supposed to prove that this depends altogether on the impulse of the heart. What are the results? When the blood in the artery is arrested, it is evident that none can flow towards the web, except what is contained within the point of compression.

The circulation in the web does not immediately cease on intercepting the *vis-a-tergo*, but, on the contrary, continues several minutes, and ceases only when the artery fails to furnish the necessary supply of blood. He indeed remarks, “The globules continue to be moved, but more slowly. This motion becomes slower and slower, and ceases entirely after two or three minutes.” The circu-

* Leçons sur les phénomènes physiques de la vie, Vol. iii. p. 269.

lution was observed for about three minutes after the interruption to the flow of arterial fluid. It gradually became feeble, until at length it stopped,—effects which might have been anticipated independently of the intercepted impulse of the heart. In the experiment the supply of blood to the web is cut off, so that the capillaries can receive only what is contained from the point of the compressed artery. They convey forward the blood as long as any is transmitted to them, and only cease to exercise their functions when the current fails. Here is a close relation between cause and effect.

Magendie calculates on the capillaries continuing in operation when destitute of the ordinary stimulus, for when they have exhausted the little he leaves them, he points to the results as proving that they cannot act without the co-operation of the heart, though he admits the continuance of circulation without such aid for about three minutes. It is difficult to imagine how he could overlook the defects of the experiment, for it may, as already stated, be urged as proving that capillary circulation is independent of the direct impulse of the heart. It is not necessary to allude to other experiments on the same subject. They are all open to the same or similar objections.

The consideration of these facts and of others much less equivocal, force upon the mind the conviction, that the capillaries are a system of vessels, through which the influence of the heart is not transmitted, in the undisturbed conditions of the animal system.

An objection to the veins receiving the direct impulse of the heart, is derived from considering their condition. Had they the relation to this organ which is insisted upon by Magendie and modern physiologists, would they not be equally distended with the arteries? If the blood flowed

directly from one to the other, an important difference in the physical condition of them would scarcely be imagined. The difference, however, is great, and acknowledged by all writers. It is remarked by Carson, that the veins "in their ordinary state, are not in the situation of rigid tubes, which they must be admitted to be, upon the supposition of the whole blood being advanced through them by a force impressed upon this fluid at their distant terminations." Magendie also states, "*Les parois artérielles sont toujours distendues par le sang, les parois veineuses sont souvent affaissées sur elles memes.*" The difference is admitted by Arnott, and he attempts to explain it in the following manner:—"The venous current is treated of as a very obscure subject; and some authors in their anxiety to explain it, have assigned causes for it, which, as will appear hereafter, are positive absurdities in physics. The difficulty in the question seems to have arisen from the great disparity observed between the tension in the arteries and in the veins, while the reflection did not occur, that it was owing to there being a free passage or outlet from the veins through the heart."

This explanation cannot be deemed satisfactory. The cause assigned is insufficient to produce the difference in the tension of the two classes of vessels. Let us imagine the tension equal; the dilatation of the right auricle removes from the upper end of the venous column scarcely an ounce and a half of blood, The difference, therefore, in the tension will be determined by the removal of this quantity of fluid. This certainly must be the measure of it. But this is inadequate to account for the marked disparity in the condition of arteries and veins; the cause, moreover, is not an uninterrupted action. If the dilatation of the auricle relieves the tension, this will necessarily

return, during the contraction of this cavity, at which time the blood has no passage or outlet. If the dilatation is the cause of the difference ; during the subsequent contraction the venous tension must be precisely the same as the arterial. The veins, however, are as flaccid in the one state of the auricle as in the other, except those in the immediate vicinity of the heart. The numerous and well-conducted experiments of Hales furnish us with exact data on the subject. The blood rose in a tube fixed to the jugular vein of a sheep, five inches and a-half ; in the carotid artery of the same, six feet five inches and a-half. There was a similar difference in all his experiments. The rise of the fluid in both vessels always varied with the muscular efforts of the animal.

The argument of Arnott is, that the pressure of a column of blood, in a descending artery, will raise the blood in a corresponding vein to an equal height. In advocating this doctrine, he evidently regards the intermediate capillary vessels, connecting the two, as offering no obstacles to the free transmission of the arterial current. In the previous pages it has been shown that the globules are not marked by a regularly progressive movement, indicating a steadily sustained pressure *a-tergo*. The contemplation of the phenomena of capillary circulation would scarcely lead to the conclusion, that these globules, either singly or in pairs, are the carriers of a pressure of 60 pounds ; and that, were it not for the alternate dilatation of the right auricle, the veins would indicate this amount. If such dilatation were the cause of the difference, it would naturally be expected that when the artery and vein were placed under similar circumstances, the results would be analogous. To accomplish this it is only necessary to give to the contents of both artery and vein a free exit. If the flow of

venous blood into the right auricle is the cause of the disparity between arterial and venous tension, provided the contents of these vessels have the same facilities of escape, it matters not whether within or without the body, the tension ought to be the same. The cause is stated to be one and the same,—the contraction of the left ventricle. According to the views of Arnott, the venous column is simply the continuation of the arterial, and differs only in being a little further removed from the propelling power; therefore, if a vein and artery be divided or punctured, the contents of each being in the same circumstances, would not the same degree of tension be observed? It is unnecessary to remark that the greatest possible disparity exists. The blood of the one is scarcely at all projected, whilst that of the other is emitted with great force, and continues to flow long after the cessation of the venous current. The blood ejected by an artery requires considerable force to arrest it, that of the vein is readily suppressed, and the current, while it continues to escape, is not in any degree modified by the successive dilatations of the right auricle.

It is important to bear in mind that the reasoning of Arnott rests principally on the experiments of Magendie. In entering upon the subject he remarks, “1. Magendie laid bare the chief artery and vein of a living animal, and lifted them at the part, so that he could make a tight bandage round the limb without including them; it was then found that the flux of blood from a puncture made below a ligature on the vein was rapid or slow, according as the heart was allowed to produce a greater or less degree of tension in the artery; this tension was regulated by compressing the artery between the fingers. 2. After a similar preparation of the parts, it is found that the blood will ascend in a tube from the obstructed vein as high as from the artery.

3. In the common operation of bleeding, when the vein is first punctured, the blood jets from it as from an artery."

This enlightened writer remarks, "after a similar preparation of the parts, it is found that the blood will ascend in a tube from the obstructed vein as high as from the artery." In the experiments of Hales, when glass tubes were fixed to the jugular vein and carotid artery of a sheep, the blood from the former rose only five inches and a-half; while that from the latter rose six feet five inches and a-half. In similar experiments on a horse, the blood from the jugular vein ascended in the tube, "in three or four seconds of time, about a foot, and then was stationary for two or three seconds; then, in three or four seconds more, it rose sometimes gradually, and sometimes with an unequally accelerated motion, nine inches more, and when the animal strained or struggled strongly, the blood rose to the top of the tube, which was four feet two inches long; but in that fixed to the carotid artery, it rose nine feet six inches *

In these cases the contents of the artery and vein have the same facility of escape, and yet how widely different is the degree of tension indicated. In the experiment alluded to by Arnott, in which the tube is fixed to the obstructed vein, the blood is said to rise as high as from the artery.

It is scarcely possible to find, in the whole range of physiological investigations, an experiment that more completely destroys the natural relations of the arteries, capillaries, and veins, than the second experiment adverted to by Arnott. The object of it is to ascertain whether venous circulation depends on the heart, or on any intermediate cause. The first step in the experiment is to

* *Statistical Essays*, by Stephen Hales, D.D., F.R.S., Vol. ii. p. 13, 4, 5.

confine the circulation of the limb to two vessels, an artery and a vein, and the connecting capillaries, so that the arterial current, urged forward by the impulse of the left ventricle, has only one mode of escape,—*through the tube fixed to the vein, and, under such circumstances, it will necessarily ascend in it, as high as from the artery.*

If the connection between the artery and vein be direct, and the sphere of circulation in the limb be narrowed to these two and the intermediate capillaries; in the experiment, from the point of the artery laid bare, to the obstructed vein will be one series of distended vessels; nor will the blood flow into the tube until such effect be produced. The distension of the vessels, as if inert in structure, is an inevitable consequence; and the value of the experiment consists simply in proving that the impulse of the heart in this peculiar condition of the vessels, is capable of being transmitted to the column of venous blood. It does not, however, in any degree, elucidate either the action of the capillaries, or the influence of causes which are imagined to act in advance of the venous current, and with what force of reasoning can it be adduced in explanation of the phenomena of circulation?

The manner in which the blood flows into the tube is not analogous to the motion of it in the veins, and yet the extraordinary disparity in its conditions has awakened no suspicion of the correctness of the experiment. The obvious effect has satisfied the inquirer, and even one eminently familiar with the principles of physical science.

The blood which flows in venesection indicates no great tension in the veins. On first making the incision, it is usually projected several feet, but it instantaneously subsides, rising perhaps a few inches, and often only trickles down the arm. The stream exhibits no jerks synchronous

with the contractions of the left ventricle, nor is it any criterion of the force employed in venous circulation. The application of a bandage to the arm previous to the operation produces distension in the superficial veins, so that the energy with which the blood escapes is the measure of ACCUMULATED pressure, and not of the degree with which it is urged forward under ordinary circumstances; the fact, however, is adduced by Arnott as an illustration of the great tension of the venous system. It is an unnatural effect, originating in a disturbed condition of the circulation.

The two systems have different degrees of tension. That of the arterial is derived entirely from the heart; the venous, chiefly from the capillaries. It is modified in both, in all muscular exertions; hence there is a primary and a secondary cause operating in both systems. Exercise facilitates the flow of arterial and venous blood; the degree of acceleration varies greatly in different portions of each system. The circulation in those parts of the body the most directly brought into play, will be the most invigorated; those less affected, in an inferior degree. The extensibility of arteries and veins will readily admit of such irregularity.

The manner in which the blood is influenced in both vessels by bodily exertion is shown by the experiments of Hales and Poiseuille. The blood which is allowed to escape on these occasions relieves at once the tension; but in the ordinary conditions of exercise, the sudden and partial tension of each system requires time for its equalization. Accordingly, a stock of motive power is gained by both, and co-operates with the primary causes in exciting and maintaining a vigorous circulation. Were the vessels full to distension, these temporary effects could not possibly

occur. It is also evident that the tension of both systems is exceedingly variable, not only generally but locally. The arterial is relieved by the fluid being urged into the capillaries ;—the venous, by its transmission into the heart, and, perhaps at times, by its accumulation in the *vena cava*, which is capable of receiving an immense quantity.

Were the tension of the venous system occasioned by the heart, the prevailing doctrine, though less than that of the arterial, would it not be expected to be uniform? The causes imagined to produce it are general, consequently cannot exercise a partial influence. The crural vein ought to have the same tension as the jugular. If the simple weight of a column of arterial fluid can raise the contents of a corresponding vein to an equal elevation; and if, indeed, a force of sixty-four pounds acts upon this column, is it possible to conceive that the veins can be partially affected? The pressure operates in every direction, hence, on this supposition, the tension in all parts of the venous system must be uniform. The experiments of Magendie and Poiseuille prove, however, that this is not the case.

A reference to their authority leads us at once to the analysis of their labours on venous circulation, which are neither few nor unimportant. In previous investigations, they had ascertained, by means of the hæmodynamometer, that the pressure in all arteries is the same, whatever be their distance from the heart. Admitting the conclusion, the tension in the venous system, though less than that of the arterial, would be expected to be alike throughout. If the pressure in the one system be uniform, the same causes being in operation in the other, varying degrees of tension, according to the situation of the vein, are results clearly inexplicable on the received doctrine. According to this, not only is there a *vis-a-tergo* of immense power, but in-

deed a force which continues undiminished in energy, even to the extreme arteries, urging the blood into the capillaries. Are not these conditions amply adequate to establish an equal tension in the veins? Magendie and Poiseuille, nevertheless, prove, that such is not the case. The difference in the tension of the two systems, and the variety observed in the venous, are thus explained by them:—

“From the experiments which we made at last meeting, there results for you the knowledge of this important fact, that the pressure supported by the veins is greatly inferior to that supported by the arteries. In the one and in the other system of tubes, the heart is always the principle of movement, which without it could not exist. Why, then, are not the hydrodynamic phenomena in all respects identical? That depends on a number of circumstances, of which several are already known to you, and the others shall be successively pointed out as we proceed in these inquiries. The arterial circulation claims to be treated by us, in the first place, as presiding over the progress of fluids in the other points of the vascular circle; and, commencing with it, we shall proceed, so to say, from the known to the unknown. Before going farther, we shall mention in a word the tubes which the venous blood traverses, in order to return to the pump. And, in the first place, if you cast a glance over the general disposition of the sanguineous canals, you will be struck with the difference in these two grand systems, presented by their mode of anastomoses and distribution. While the blood is projected by the heart in canals decreasing in dimension, it returns to this organ by canals constantly increasing in diameter. It is impossible that the passage of fluid columns, in spaces straiter or more ample, shall neither augment nor diminish the rapidity of the current. In the arteries

it is at their origin, in the veins at their termination, that the current is more rapid.

“The arterial parietes are always distended by the blood; the venous parietes are often collapsed upon themselves. The former being thick and resistant, uneasily re-act upon the liquid column: the latter, thin and flaccid, enjoy but a feeble degree of elastic spring. In these you will meet with numerous valves, destined to oppose a reflux of the currents in certain directions, while those present not the least vestige of such a structure in their whole course. There is a uniformity of pressure throughout the general arterial system; but a very great variety of pressure in different portions of the venous system: in each system respectively, the fluid, endowed with very different physical properties, moves very differently: in the one there is a rapid, in the other a sluggish motion of the sanguineous column. When you open an artery, the stream escapes by a succession of jets synchronous with the pulse; on a vein being opened, the stream, if there is any, is feeble, and glides out with a uniform motion.

“Above all, it is in the number and capacity of their tubes that these two departments of the vascular apparatus essentially differ from each other.”*

In this extract, there is no notice of the cause, which is considered by Arnott as alone sufficient to explain the general disparity between the tension of the two systems, nor does he allude to the labours of these physiologists on this interesting branch of inquiry. His principles would scarcely lead him to imagine, that every part of the venous system had different degrees of tension, springing out of the uniform pressure of the arterial. None of the

* *Leçons sur les phénomènes physiques de la vie.* Tome iii., p. 148.

conditions stated will explain the results at which these physiologists experimentally arrive, the general accuracy of which it is not intended to call in question. A disparity in the tension of the two systems will be admitted, but this is not explicable on received views

It is now necessary to proceed to the analysis of the causes adduced by Magendie as accounting, not only for this difference of tension, but for the variety observed in the venous system; which causes may be conveniently arranged under eight heads:—

1. The heart is alluded to, as urging the blood forward at a great rate in the arterial system, from acting on the column in immediate contact with it.

The velocity* of circulation is no part of this inquiry, but simply the tension in the two systems. Magendie and Poiseuille inferred, from experiments on arterial circulation, that though the blood became slower as it proceeded from the heart, the same pressure was manifested in an artery in the thigh, as in one in the vicinity of the left ventricle, and, therefore, the rate of motion is clearly no subject for consideration at this moment.

2. Arteries and veins differ greatly in their anastomoses and distribution.

This is admitted. The anastomoses of the veins exceed those of the arteries, and the number and capacity of the veins are also considerably greater; but what relation have these circumstances to the present inquiry concerning tension? If the blood in one class of vessels, is an uninterrupted column extending to another, the pressure will

* In experiments, which we have previously analyzed, he had proved, that though the blood moved with different degrees of velocity in all the arteries, whatever might be their size, it exhibited the same momentum.

be equally distributed. It is a principle in hydrostatics, that, "in a quantity of fluid submitted to compression, the effect is equally diffused throughout the whole, and similarly in all directions." The increasing capacity of the arteries, from their frequent subdivisions, and the diminishing capacity of the veins, in their course towards the heart, will influence the rate of motion, but not the amount of pressure transmitted by the left ventricle.

3. The arterial fluid is rapid at its commencement, and the venous at its termination.

The disparity in the capacity of the two systems and their different relations to the central propelling power, explain these phenomena.

4. It is stated that the arteries are always distended, and the veins generally flaccid.

Does this show the equalization of pressure ?

5. The arteries re-act powerfully on their contents, the veins scarcely at all.

The motion of the blood would be modified by this circumstance, but the pressure permanently acting from behind will neither be increased nor diminished by it. The force of the heart, spent in dilating a contractile vessel, is said to be restored by its re-action. Had it neither this property nor dilatability, in place of the propulsive power being thus momentarily lost, the forward rush of the fluid would of course be greater. In a contractile vessel, the power is spent in two directions,—laterally and longitudinally ; in one that is not contractile, in the latter direction only. . If the venous system be full and directly connected with the arterial, the contents of the former will be urged forward by the pressure in the latter ; if not full, and so connected, what is to prevent the blood from establishing an equal or uniform tension in the veins ?

On the views of Magendie and physiologists generally, the contractility of the arteries cannot be regarded as an important modifying condition. The impulse of the left ventricle is stated to be transmitted throughout the venous system ; therefore, what is to prevent this impulse from establishing an equality of tension in both systems ? The cause assigned is, that the vessels of the one are endowed with contractile properties, in virtue of which they re-act on their contents ; while those of the other admit of great distension, and possess little contractility. The absurdity of this argument will appear, on a very slight consideration, keeping strictly in view the reiterated assertions of physiologists, viz., that the capillaries connecting the two systems perform no part in the process of circulation, being simply the carriers of the blood, and originating no motive power.

Supposing no obstructions to exist between the two systems, which, of course, is implied in the doctrines of these physiologists, the impulse of the left ventricle would inevitably establish an equality of tension in both. The blood would flow out of the one into the other, until such effect were produced, and then every successive contraction of the ventricle would maintain the same tension in both classes of vessels. The blood in the veins cannot possibly be propelled by this power, unless both vessels be in the same state.

The arterial column cannot push forward the venous without this necessarily causing distension in the vessels along which it flows, and this condition would be general and modified only by the quantities removed by the successive dilatations of the right auricle, but which quantities would make no difference in the rounded appearance of the veins. In the experiments of Magendie, in which he

attempts to prove the direct influence of the heart on venous circulation, the blood does not escape from the vein exhibiting such influence, or in harmony with the arterial current, until it becomes thoroughly distended. The same occurs also in showing the direct connection between arteries and veins, by the injection of water in the dead body. No difference in the properties of the two classes of vessels would prevent the same effects being induced in both, in the natural conditions of the animal system, reasoning on the doctrine that the influence of the left ventricle is transmitted directly to the venous column.

6. There are valves in the veins, and none in the arteries.

The valves are to prevent the retrograde motion of blood, and have no effect on its progression, or influence on the causes acting *a-tergo*, and consequently may be left out of consideration.

7. The properties of the blood are different in the two systems.

They are much more stimulating in the one than in the other. This may cause a difference in the rate of circulation, but none in the tension of the vessels. The tension has a reference to the power which propels and not to the quality of the fluid.

8. There is a great difference in the capacity of the two systems.

The influence of this circumstance has already been considered.

Magendie, after enumerating the foregoing conditions, concludes as if the inference were unavoidable.

“There is a uniformity of pressure throughout the general arterial system ; but a very great variety of pressure in different portions of the venous system.”

The inference does not flow from the facts stated. The cause of the various degrees of pressure in the venous

system is altogether unexplained on these conditions. Previously, however, to entering on the consideration of it, attention is directed to the following remarks by this physiologist, exhibiting, very limited views of the relation between different parts of the circulatory system.

“If the movement of the blood in the veins were solely owing to the action of the capillaries, the ascent of the fluid would necessarily be uniform, and not in accordance with the causes which augment the force of the arterial blood. The degree of the heart’s energy, the respiratory motions, and the volume of the fluid in that case, would exert no influence on the venous circulation. But this is opposed to the testimony of daily observation. It is known for certain that what acts on the arteries acts also on the veins.”*

It is here stated, that were the capillaries the cause of venous circulation, this would be uniform, bearing no relation whatever to the powers which increase the arterial; nor would the degree of energy with which the heart acts, the character of the respiration, or the quantity of fluid to be moved, exercise any influence on the contents of the veins. The strangeness of this opinion accords little with the established reputation of the writer. Suppose the capillaries to circulate the blood they receive, without the co-operation of the heart and arteries, being themselves a source of motive power, they would clearly be liable to modifications according to the energy of the powers acting *a-tergo*, and the stimulating qualities of the vital fluid. Larger the volume of blood transmitted in the direction of the capillaries, and of course greater the quantity they would receive and circulate. Their functions are

* Leçons sur les phénomènes physiques de la vie. Tome iii. p. 143.

capable of being either excited or depressed. If the amount conveyed to them be less than usual, the veins will receive less. When the arterial circulation is the most active, the venous, according to the views advocated in these pages, will always necessarily correspond. The two in health will ever be in strict and harmonious relation. The contents of both systems are accelerated by the same general causes; the one towards the capillaries, the other where the least pressure exists, which is mostly in the direction of the heart. The want of uniformity in the tension of the venous system is explicable only on capillary influence. I proceed now to the examination of his experiments, proving, in his own words, “*tandis que la pression uniforme dans la generalite du systeme arteriel elle varie dans chaque partie, et pour ainsi dire, dans chaque tuyaux du systeme veineux.*”

The instrument used by him has already been described. Its invention is due to Poiseuille. In previous strictures it was stated to be admirably adapted to determine the force of the heart, but incapable of indicating the momentum with which the blood moves in different parts of the arterial system. The one question is comparatively simple and easy of solution; the other is abstruse, exceedingly difficult, and perhaps not admitting of elucidation. The application of it to venous circulation is not free from objection, but it is sufficiently correct for my purpose. The instrument, indeed, will not show the force ordinarily employed in the motion of venous blood, but will indicate the relative differences in its momentum in various situations. If the venous system be imagined to be divided into ten lesser systems, and an independent heart be regarded as propelling the vital fluid in each, the instrument applied to the vessels of these systems will exhibit the propulsive energy of the several

hearts, but not the force with which the blood moves in its ordinary or undisturbed condition in each of these systems.

It has already been remarked, that when the course of the arterial blood is arrested, this acquires relations to the propelling power either altogether new, or exceeding what is natural, and the truth of this observation will subsequently be obvious. Suppose the instrument to be fixed in any large vein, the blood cannot flow forward or escape from the pressure acting *a-tergo*; the only play of which it is capable, is derived from pushing the mercury in the hæmodynamometer a few lines in advance. If the ligature applied to the arm in venesection causes the blood to accumulate and to be projected with increased energy, this instrument, obstructing, in a similar manner, the venous current, will produce analogous results; and, therefore, will not afford any measure of the momentum of it in its natural circumstances; at one instant accelerated, at another retarded, or perhaps moving in a retrograde direction; and, moreover, the current, during the experiment, is withdrawn from the influence of one class of causes, to which some writers attach great weight, viz., the enlargement of the chest on inspiration and the dilatation of the right auricle. None of these conditions have been considered, nor was the possibility of their occurrence even suspected by the enterprising physiologist. He has conducted his numerous experiments, as if the powers of life were as simple and easy of appreciation as the laws of purely physical machines. The modifying influence of vital properties has not only been discarded from his calculations, but any attention to it is treated with contempt and ridicule.

Understanding the capabilities of the instrument, the reader is prepared to examine the results which it furnishes.

The numbers in which they are expressed require no explanation nor comment, except that they represent quantities which are measured by the millimetre, which is the thousandth part of the French metre, of about $39\frac{3}{8}$ English inches. The instrument is applied to the left jugular vein of a dog, and gives the following results: 15, 20, 14, 18, 15, 17, 18 millimetres.

When the animal struggled or made powerful expirations, the mercury suddenly rose to 35 and 40 mill., but immediately fell, on the respiration becoming natural, to 15, 20 millimetres.

It is subsequently fixed to the crural vein, and the mercury oscillates between 55, 60, 50, 45, 50, 55, 58 mill.

The difference between the pressure in the two veins is here shown to be very considerable. In the one the pressure is nearly three times as great as in the other, and yet it is referred to the same impulsive powers—the action of the heart and the contractility of the arteries.

He afterwards applies the instrument to the internal saphena vein, which is small, and the numbers were 20, 22, 19, 22, 20. On compressing the deep-seated saphena vein, so as to arrest the circulation in it, the mercury rose to 25, 28, 30, 32 millimetres. On ceasing to compress, it again fell to 19, 21, 19, 20.

On compressing the vein, the sphere of circulation was of course narrowed. The instrument obstructs the motion of blood, giving rise to the increasing distension of the vessel; consequently the pressure on the mercury will be proportionably augmented, which is indeed the fact. Such might have been predicted from knowing the effects of a bandage round the arm in venesection. What does the experiment prove beyond this simple and evident phenomenon? Can any other explanation be proposed to

account for it? The same experiment is performed on the jugular veins. The instrument is fixed to one of the veins, and a ligature is afterwards applied to the other. Before the ligature is tightened, the mercury rose to 15, 17, 15, 16, 17, 16 millimetres. When tightened, it rose to 20, 25, 23, 26, 25. On removing the ligature, it again fell to 15, 16, 15, 17, 16.

These results are neither particularly interesting nor important. The experiments show, what no experiment was required to establish, that if the blood cannot flow in one direction, it will escape in another; and further, when arrested in its course, that the tension in the vein where it accumulates will be increased.

The physiologist next endeavours to prove, that when the arterial fluid sent to an organ is diminished by tying a principal artery, the venous circulation is affected. The experiment, however, does not present any remarkable differences.

The left external jugular vein is tied, and the instrument is connected with the right. The right carotid artery is then compressed. The numbers indicated by the ascent of the mercury were, 20, 22, 20, 21, 19 millimetres. On ceasing to compress, they were, 20, 19, 20, 22, 21.

In this case there is no appreciable difference. To render the experiment more decisive, the artery is tied in place of being compressed. The results, however, are nearly the same. The left carotid artery is afterwards compressed, so that the brain receives blood from the two vertebral arteries only. On compressing the artery, the mercury stood at 15, 17, 14, 16, 15, 16 millimetres. On ceasing to compress, it rose to 20, 22, 19, 21, 20, 22 mill.

The difference even then in the degree of pressure indicated by the vein is very trifling. But what does the

experiment demonstrate, or what is the value of the results to which it can possibly lead? It clearly shows that, when the quantity of blood usually sent to an organ is lessened, the vein corresponding to the tied or compressed artery receives a diminished supply, hence the tension in it is weakened. This is the only fact the experiment can establish. The modification in the tension of the vein does not prove the direct influence of the heart upon the venous column. If the circulation in the veins depended on this cause, on the withdrawal of the influence, the motion of the blood would naturally be expected to be arrested. In the last experiment detailed, however, no vital fluid, and consequently no impulse is transmitted to the brain through its two principal arteries, and yet the difference in the pressure indicated by the instrument is little more than fractional. When one artery only was compressed, the difference was not appreciable. Are these results such as would be anticipated from the doctrine that venous blood moves only in virtue of the direct impulse of the heart and the contractility of arteries? Were they much more decisive in their character, they would still be extremely questionable as data on which to raise just and comprehensive views. Four important vessels, two veins and two arteries, cannot be laid bare without acute suffering to the animal, and the torture is not momentary, but continues during the whole of the experiment. In addition to the pain which always modifies the vital phenomena, the ordinary conditions of cerebral circulation are disturbed quite as much as is compatible with life. The marvel is, not that a slight difference in the tension of the vein is observed under such circumstances, but that there is sufficient vitality to afford any indications of the laws or principles of life. Amidst this multitude of disordered actions, where

is the eye or understanding capable of separating the natural from the induced conditions ?

There is one other experiment to which it is necessary to allude on this occasion. Magendie was not fully satisfied with the results of the preceding experiments. He had not succeeded in intercepting the transmission of blood to the brain, which he deemed desirable in order to determine the pressure in the vein connected with the hæmodynamometer under such circumstances. The thigh appeared the best adapted to answer the object of his research. Here it is not difficult to confine the circulation to two vessels, the crural artery and the crural vein. The latter, with the exception of a few branches, returns from the limb the blood conveyed to it by the former. When the branches are not compressed, the mercury oscillates between 55 and 60 mill. When the branches are compressed, so as to force the whole of the blood into the vein, the scale indicates—80, 85, 75, 85, 80 mill. On ceasing to compress, the mercury fell again to—62, 55, 58, 60, 58 mill.

Having ascertained the pressure in the vein, when the flow of arterial fluid to the limb is not interrupted, he compresses the crural artery so as to intercept completely the impulse of the heart. The tension in the vein is then observed to be 55, 50, 45, 42, 35, 33 mill. On allowing the blood to re-enter the artery, the mercury rose to 62, 60, 63 mill. On compressing the artery, the mercury again fell to 55, 48, 45, 40, 36 mill.

This experiment is intended to prove, that the pressure in the venous system is wholly referable to the direct impulse of the heart. Does such inference flow from it ? When an artery is compressed, or a ligature passed round it, the blood continues to circulate towards the capillaries, and leaves it empty. In this case there is no column in

the artery capable of raising the contents of a corresponding vein to an equal height. The blood having passed on to the capillaries and the vein, the tension in the latter clearly cannot be referred to any cause existing between the arterial capillaries and the left side of the heart. There is neither the impulse of this organ acting *a-tergo*, nor the column of blood in the crural artery forcing this fluid along. Nor can it be attributed to any property of the vein. This is only a recipient and carrier of the blood, There is then only the capillaries that can exercise any influence. This experiment, but for a different object, had previously been performed by Magendie, and the results at which he arrived established the independence of the capillaries on the direct impulse of the heart. The experiment just analyzed shows that the tension in the vein is almost as great when the artery is compressed, as when perfectly free. The other experiment referred to proved that the blood continued to flow from the punctured vein, when the impulse of the heart was wholly intercepted. The two establish the same fact, and corroborate the same train of reasoning. In the following investigations it will be shown, that the diversity of venous tension is explicable chiefly on the action of the capillaries.

CHAPTER III.—THE CAUSES OF THE VARIETY OF TENSION IN THE VEINS.

In the preceding researches the object has been to prove that the experiments of physiologists do not establish the direct impulse of the heart on venous blood. The evidence which they present may be adduced in refutation of the opinion, and with so much force, that it is difficult to explain the prevalence of it. The difference in the tension of arteries and veins has always been regarded as rendering the phenomena of circulation exceedingly abstruse and embarrassing. The one class of vessels is admitted by all to be distended, and the other to be flaccid, hence capable of receiving a much greater quantity of blood than they ever possess in health. There are, also, other important distinctions. The one re-acts on the vital fluid, the other can scarcely be supposed to exert any particular influence. The re-action of the arteries is attributed to previous distension, and the momentary cessation of the cause which produced it. The circulation in the veins, according to the description of Magendie, is slow and equable, not exhibiting the synchronous impulses of the heart; consequently, the veins are not one instant dilated, and the next contracted. The addition to the arteries is

not only made at times corresponding with the systole of the left ventricle, but in considerable quantity. That to the veins, even on the views of Magendie, is made up of finely attenuated currents, or strings of globules, moving irregularly forward. The two systems of vessels are obviously very differently circumstanced. Re-action, in the one, is, to a certain degree, inevitable; in the other, it is scarcely possible to imagine the agency of such a power. The tension in the veins is referred entirely to the pressure of the arterial column. No obstacles are conceived to exist between the two systems of vessels capable of intercepting the force *a-tergo*. If the dilatation of the right auricle is the cause of the diminished tension in the veins, its influence can be exerted only when it acts.

Suppose a system of inert vessels, having the capacity of the arteries and veins, and connected by exceedingly small tubes, offering no resistance to the pressure from behind, so that the tendency of it will clearly be to establish an equality of tension. Whether those representing the veins be ten or fifty times more numerous than those bearing the relation of arteries to them, does not at all affect the hydrostatic principle. Into the latter, a fluid is injected at regular successive periods, and from the former, a proportionate quantity is expelled. If the whole series of vessels be imagined full, and an ounce and a half be added at one extremity, an equal quantity will be displaced from the other. If the vessels are not full, the fluid will of course accumulate until they are, and then the escape and introduction of it will be synchronous in time, and proportionate in quantity. Conceive further, that with these tubes is connected an apparatus, by means of which an intermittent is converted into a continued stream, so that the fluid is in constant motion. This

modification in the system of inert vessels does not interfere with the equalization of pressure. The force momentarily lost in compressing a reservoir of air is immediately restored by its re-action. The quantity of fluid escaping from one class of these vessels will be exactly proportionate, in equal times, to that which enters the other; and no continued stream could be kept up, unless both were full.

Were a valve placed at the end of the large tube, supposed to represent the *vena cava*, and opened only at times, corresponding with the impulses of the piston, re-action of the reservoir could not take place in the interval. This requires an uninterrupted exit for the fluid. The condition into which the reservoir is put, in this imaginary case, arises from the piston expelling, in the first instance, a less quantity than it adds. This disproportion in the two quantities is quickly corrected, and then the reservoir remains permanently compressed, exerting no more influence on the fluid than would a mere bulging of any of the tubes.

The vessels in the animal system are not thus circumstanced. The blood is continuous in its motion, and the apparatus by which this is effected is very different from the one described. The tension in the arteries, according to prevailing views, can vary little in the systole and diastole of the heart. Though momentarily distended by the former, re-action in them is calculated to maintain a corresponding condition; hence an uninterrupted pressure is urging the blood towards the capillaries. The dilatation of the right auricle removes from the extremity of the venous column an ounce and a half; consequently, in the subsequent contraction, diminished tension exists in this situation.

It is a principle that compressed fluids will rush in every direction until an equilibrium be established. Accordingly, the venous blood will at once flow towards the right auricle, while contracting. It cannot be supposed that the space will only be partially occupied. The loss of the small quantity of fluid transmitted forward will be fully compensated in the time required for its removal. At this moment the elastic contractility of the arteries is said by Magendie, Poiseuille, and others, to force the blood in this direction : the effect which is here insisted upon is therefore inevitable. A diminution in the tension of the *vena cava* will be felt by the complicated series of venous tubes. A cause which influences the whole cannot be regarded as the agent of partial results. According to this reasoning, the venous system has two periods in which different degrees of tension exist, and both equally universal ; one during the contraction of the right auricle,—the other during its dilatation. The veins, however, throughout the body are as flaccid in the one state of the auricle as in the other. Were these vessels full, and kept in this condition by the pressure of the arterial column, equal to sixty-four pounds, the removal of an ounce and a half of blood would affect only momentarily the venous tension.

But even on the arguments of these physiologists, it may be shown that the direct impulse of the left ventricle is not transmitted to the venous column. Were it, indeed, to urge this forward, accumulation of blood and cessation of circulation would occur. The re-action of the arteries, co-existing with the contraction of the right auricle, forces the blood into the *vena cava*, to supply the loss occasioned by the previous dilatation of this cavity. Were the influence of the left ventricle, also, subsequently transmitted to the venous column, a greater quantity of blood would be deter-

mined to the right side of the heart than could possibly be removed. The diminution of tension in this situation occurs when this ventricle is dilating, at which time the venous column is imagined to be moved forward, by the re-action of the arteries, to furnish to the auricle, in its next dilatation, the required quantity of fluid. Were the contraction of the left ventricle to send an additional quantity, there would clearly be two causes in operation, each of which is sufficient to supply the necessities of the right auricle. It may, perhaps, be contended, that the quantity supplied and urged forward by the ventricle is essential to maintain the balance of circulation between the two systems. This is undoubted. The quantity of blood removed from one end of the column must be furnished by the other, but this may be as accurately accomplished by the capillaries, as by the direct impulse of the heart. This is a question very different from the one under consideration.

A few remarks will render my views on this subject more clear. To the understanding of it, it is necessary to keep in mind the prevailing doctrine of physiologists respecting the motion of blood in arterics. Two causes, acting consecutively, are insisted upon,—the impulse of the left ventricle,—and the elasticity of the arteries. The left ventricle contracts when the right auricle is dilating, so that, agreeably to received notions, this impulse, acting on the arterial column, will urge forward a quantity of venous blood adequate to the co-existing wants of the right auricle. It is scarcely possible to deny this, when it is considered that the dilatation of the auricle, by the diminution of tension at the venous extremity, facilitates the flow of blood. If an ounce and a half be removed by the dilatation of the cavity, during the subsequent con-

traction of it, this loss will be compensated by the re-action of the arteries,—the second propulsive power brought into play when the left ventricle is dilating.

What is to prevent this second power establishing an equilibrium of tension, by transmitting into the venous system the ounce and a half of blood removed by the dilatation of the right auricle? To question the fact, is to argue that a fluid in motion will not flow where it meets with the least resistance.

The doctrine which is here combated derives the whole of its plausibility from the supposition, that the force of the left ventricle is expended in two directions,—the one in urging the arterial column forward,—the other in dilating the arteries; so that this force is divided into two halves, which conjointly send into the venous system the ounce and a half of blood removed by the dilatation of the right auricle. A brief analysis of this opinion will at once expose the absurdity of it. When the left ventricle contracts, the co-existing dilatation of the right auricle not only allows, but facilitates, the flow of blood in such direction; and it would be strange, indeed, reasoning on the views of Magendie, if it were not at this moment urged forward, to establish an equality of tension in the venous system.

If the dilatation of the auricle removes an ounce and a half of blood, during the subsequent contraction, the arteries,—the second propulsive power, are then returning upon their contents with the force originally expended in the dilatation of them; and what is to prevent this power establishing an equality of tension in the venous system, imagined to be disturbed, by the removal of an ounce and a half of blood from the venous extremity?

To do justice to the arguments which are here employed,—the reader must not lose sight of the doctrine of Magendie,

and of physiologists generally, viz. that the force of the left ventricle pushes forward at every contraction, the venous column, precisely as the piston of a syringe does the fluid on which it acts,* and, in the animal system, this occurs when the right auricle is dilating, thereby allowing an uninterrupted course to the forward rush of the blood.

At the next moment, when the right auricle contracts, to expel the ounce and a half of blood, the elasticity of arteries comes into play; and, if there be any diminution of tension at the venous extremity,—from the removal of that quantity of blood, will not this power instantaneously re-establish the previous existing condition.

Magendie states that the pressure in the veins is not the same in any two. The variety is certainly not explicable on hydrostatic principles. If fluids find their level when compressed, the blood will be no exception to the law. In treating of the different results furnished by the hæmodynamometer, he alludes to the influence of the numerous anastomoses of the veins. Whether the veins have 50 or 500 branches cannot affect the distribution of the blood. The pressure must be universally the same, whether the cause be the impulse of the heart, or the contraction of the arteries.

Whether the anastomosing vessels be 50 or 500, they are necessarily in the same condition. To argue otherwise, is equivalent to asserting, that some of these vessels are only partially filled, while others are in a state of distension, which manifestly would be an absurdity. To perceive the full force of the objection, let the inquirer simply ask what are the imagined causes of venous tension? The answer will be, the heart and the contractility of the arteries. To

* *Leçons sur les phénomènes physiques de la vie. Tome iii., p. 37.*

contend that either of these causes produces a different degree in each vein, is not less strange than the doctrine would be, that the stroke of the piston occasions a variety of tension in the several tubes through which the fluid is propelled. To relieve the pressure at the extremity of the venous column, is to affect equally the whole of the venous system. The cause in this instance is not less general than the heart or the arterics, and consequently it would be unphilosophical to expect partial results.

In these remarks the object has been to show that, according to the received views, the blood removed by the dilatation of the right auricle is immediately compensated by the ascent of the venous column, not from the direct impulse of the heart, but from the pressure of the arterial column; hence the force of the left ventricle cannot be required to extend its influence to this distant point.

The connection between the two columns of blood is imagined to be the same as exists between the two columns in the system of inert tubes, and is regarded as established by the same fact, viz., if fluid be injected into one class of vessels, it must be expelled from the other. This would certainly be the case. Both are in this respect alike, and appear to rest on the same evidence. There is danger, however, in analogy, and especially when traced in phenomena originating in very different causes, and influenced by very different circumstances. The two columns in the animal system are connected, but not in the same manner as in the apparatus in question. The capillaries absorb and propel blood when the heart has ceased to contract, or the agency of it is altogether interrupted. Were the action of the piston to be arrested, the fluid in the descending limb of the apparatus would not be drawn by intermediate

tubes into the ascending and corresponding limb and be *expelled*. It would remain at rest.

Hence there is an important distinction in the properties of the minute and intermediate vessels connecting the two columns in both cases. In the one, there is evidently a cause in operation sufficiently active to convey forward the fluid into the ascending vein; in the other no such power exists. Here then the analogy in the condition and functions of the vessels uniting these columns entirely disappears. There is another striking peculiarity. However small the quantity of fluid injected by the piston, a proportionate quantity is emitted. Accumulation is impossible. Now, the quantity of blood sent into the aorta by the impulse of the left ventricle is not necessarily removed by the dilatation of the right auricle. The two cavities will always have a corresponding number of contractions, but not an invariable relation in the quantity expelled and received in a series of consecutive actions.

The consideration of this peculiarity opens a wide and interesting field of inquiry. Were the right side of the heart always to remove what the left transmits, congestion would never take place. This, in fact, arises from the inequality in the action of the two cavities, and strongly marks the difference between the motion of fluid in inert and organized vessels.

Were venous circulation dependent on the impulse of the left ventricle, is it not extraordinary that this impulse should allow blood to stagnate in its natural channels? If the weight of the arterial column is alone sufficient to raise the venous to the heart, the additional force of sixty-four pounds, acting equally in all directions, would surely prevent the accumulation of blood in the veins. The phe-

nomenon can only be accounted for on the supposition, that the combined propulsive powers are inadequate to move the venous column, or that its condition depends on the influence of the capillaries. Either supposition is unfavourable to the prevailing views of physiologists. If the direct impulse of the heart is the cause of venous circulation, here is clearly an instance in which it is incapable of urging the blood forward. According to these views, as long as the congestion exists, the impulse is directed against the interrupted columns, but in vain. Whatever may be the amount of the *vis-a-tergo*, it is evident that accumulations occur which this is inadequate to overcome, and remain stagnant until dissipated by local remedies or constitutional changes. These are facts of great value in this inquiry. Both the congestion and its removal indicate the operation of powers which cannot be attributed to the heart. The piston cannot cause an unequal distribution of fluid, nor can the heart either produce this or correct such condition.

These effects are inexplicable on the views which are here combated. The phenomena admit only of one explanation, viz., the independence of capillary circulation on the direct impulse of the heart. The blood accumulates in defiance of the immense force which is said to give it motion, and the subsequent equable distribution of it takes place from changes in the capillary vessels, and not from any modification in the character of the ventricular impulse. These facts demonstrate that the right and left cavities of the heart, though synchronous in their respective actions, do not always correspond in the quantity of blood received and transmitted by them. Congestion occurs when the left side conveys into the circulatory system a greater amount than is removed by the right, and

it is dissipated only by the subsequent preponderating activity of the latter. This irregular or unequal action in the cavities of the heart, shows that the proposition which is usually laid down, in regard to their perfect harmony, cannot possibly be adopted. The strict relation supposed to exist between them is frequently and seriously disturbed. No further argument is required to prove the want of analogy between the circulation of blood and the motion of fluid in inert tubes.

It is not difficult to explain the seeming uniformity in the quantities of blood propelled and received by the right and left cavities of the heart, nor the occasional inequalities exhibited. The phenomena are satisfactorily accounted for on the functions of the capillaries. The various organs of the body are not supplied, even in health, with an invariable quantity of blood. There is scarcely one which has not its periods of activity;—the brain, during the intellectual exertions of the day,—the digestive apparatus, after the reception of food,—the generative system, during its important modifications. The quantity of blood required is always proportionate to the energy of the individual functions; hence the distribution of it is constantly varying in different parts of the animal system. These particular changes are not occasioned by the impulse of the heart. This is incapable of producing partial effects.

The capillaries draw from the general sanguiferous system the necessary supply, and this varies according to the urgency of the local wants. Severe and long-continued mental exertions cause an excited cerebral circulation, and were our means of investigation refined, it would, perhaps, be discovered that a greatly increased quantity of blood is deprived of its spiritual properties.

The undue or vigorous action of several important organs is seldom observed at the same time; illustrating the economy of nature. Their consecutive activity secures an abundant supply of blood which could not otherwise be furnished. The constant modifications in the distribution of it are inexplicable on the impulse of the heart and the contractility of arteries. They are evidence of the independent influence of the capillaries. If such can increase or diminish the amount of blood in an organ, on what grounds can it be denied that they affect the motion of it in the veins?

The inquiry into the functions of the capillaries will account satisfactorily for the variety of tension in the veins. It is stated by Magendie, that no two have the same degree, and this result, so unexpected from the tendency of compressed fluids to establish an equilibrium, is in perfect conformity with the views which it is my object to develope. Every organ has its own independent system of capillaries. The blood which it receives is modified both in properties and circulation, according to the nature of the function. The difference in the vital actions must be mainly attributable to the capillaries; and the effects of their operation are different in each organ. Le Gallois has shown that the venous blood returning from the several viscera is not identical in its properties. Though supplied with the same arterial fluid, the processes and effects in each are peculiar. In one, this fluid is essential to the mysterious operations of the mind,—in another, to the secretion of urine,—in a third, to the formation of gastric juice; and to various other important actions. The mechanism of the organ, or the structure of the capillaries, explains these different results. Each viscus has its independent capillary system, in which both the

properties and the circulation of the blood are modified. It is illogical to admit one and deny the other; for that which causes a change of properties depends upon, or gives rise to, a peculiar circulation.

The modifications which the blood undergoes in the capillaries shows how inaptly organic vessels can be compared with inert tubes. The one produces two important results,—a change in the properties and the circulation of the fluid;—the other produces neither of these effects. Guided by views, which, indeed, mainly rest on experiments, the different degrees of tension in the venous system are easy of explanation. The capillary systems are new sources of motion, and as they differ in each organ, in regard to structure or disposition, the blood flowing into the veins will be carried forward with a pressure corresponding with the forces in action. How far these forces are modified by the situation and functions of the capillaries it is impossible accurately to determine. If permitted, however, to speculate on the subject, it may be inferred, that where the functions are exceedingly refined in character and free from the direct influence of bodily exercise, the venous blood will flow with less force than from organs, the seat of grosser vital processes, or processes liable to be excited by frequent muscular contractions. The tenuity of the vessels will bear a relation to the delicate offices performed by them. The cerebral capillaries would scarcely be imagined to be the same in character as those of the thigh. In the one situation, an immense quantity of blood is distributed for the purpose of maintaining the elaborate and ceaseless operations of the brain; in the other, the functions are such as are largely essential to the organic existence of the part. The vessels of the latter are in a great measure only carriers of the

vital fluid. These are important distinctions, and are associated with corresponding differences in the tension of the veins directly connected with them. According to Magendie, when the hæmodynamometer was applied to the crural vein, the mercury oscillated between,—55, 60, 63, 58, 60 millimetres. Applied to the jugular, the mercury rose only to,—15, 17, 15, 16, 17, 16 millimetres. He attempted, in both cases, to render the results more remarkable by circumscribing the circulation. In the thigh, the whole of the blood conveyed by the femoral artery was made to return by the corresponding vein. The hæmodynamometer then indicated,—80, 85, 75, 85, 80.

The instrument was placed in the right external jugular vein, and a ligature was passed round the left, so that, with the exception of the small quantity of blood returned by the internal jugulars, the whole was directed against the hæmodynamometer. The mercury, however, even then rose only to,—24, 23, 20, 23, 25, 24 millimetres. This is a very slight increase on the results obtained previously to the application of the ligature. These two experiments are important. They show that, when the blood is placed in analogous circumstances, the tension in one vein is almost four times greater than in another. Such differences are inexplicable, on the doctrine that venous circulation depends altogether on the impulse of the heart. In both experiments the motive power is concentrated and confined to one channel—leading to the instrument, and yet the disparity in the tension indicated is extraordinary: they may be adduced in corroboration of the conjectures advanced, respecting the peculiarities of the capillary systems and their corresponding influence on the motion of venous blood. Magendie found venous tension to vary greatly in all parts of the body, and so far from this being a startling

and unexpected result, it might have been predicted from the important functions of the capillaries and their independence of the direct impulse of the heart.

The next step is to endeavour to explain the seeming relation in the quantities of the blood received by the left ventricle and right auricle. It has already been stated that frequent inequalities exist in regard to these quantities. Congestion occurs when one cavity conveys more than the other receives in equal times; and it is subsequently removed by the translation of the preponderating influence. The amount of the inequalities is no part of the consideration. The fact alone concerns the inquiry. The capillaries are admitted to be the seat of all vital actions; and it is equally incontrovertible, that the quantity of blood used or deteriorated by them, in any given period, may be considered as measured by the quantity of blood sent into the aorta by the left ventricle in the same time. When these actions are excited, they require a proportionate increase in the supply. When enfeebled, the demand diminishes accordingly. In the one case, the contractions of the heart become strong and frequent; in the other, weak and often quick. These actions are, indeed, the regulators of the quantity received by the two sides of the heart. They maintain a general and necessary relation between them. When depressed, they are incapable of conveying forward the whole of the blood sent by the left ventricle, and consequently a portion is retained in some part of the circulating system. The lessened quantity sent into the veins is the measure of the amount of vital action existing; and if the depression be great and of long continuance, this quantity gradually diminishes; and, at length, the ventricle and right auricle transmit and receive corresponding quantities.

If a part of the vital fluid is arrested in its course, a less quantity will pass from the right to the left side of the heart. Extensive congestion affects both the capillaries and the large internal veins. There is no scarcity of blood at the right side of the heart, but, on the contrary, accumulation; so that the diminution in the quantity received by the right auricle is not attributable to any deficiency in the vicinity of it, but to the enfeebled action of the cavity, arising from the deteriorated qualities of the fluid. This circumstance is important to bear in mind, showing that though the relation in the quantities received and transmitted is maintained as in health, the diminution in the quantity received by the right auricle is not referable to the weakened impulse of the left ventricle. The one is not, indeed, the immediate effect of the other.

According to these views, the relation between the cavities of the heart is not fixed or invariable, but is liable to modifications, and such as would never occur were the pressure of the arterial column the cause regulating the ascent of the venous. It has been stated, that the small quantity of blood flowing into the right auricle during the existence of congestion is the measure of the vital actions carried on in the capillaries, and though proportionate to what is injected into the arterial system, it is not displaced by it, as in the case of inert tubes, nor do they stand to each other in the strict relation of cause and effect. The capillaries alone regulate the quantity of blood received and transmitted by these cavities, maintaining in health what may be designated the balance of circulation; and giving rise in all morbid conditions to important modifications in the quantity and distribution of the vital fluid. If these views, and the arguments founded upon them, be correct, it is evident that the principles of physical science have

hitherto failed in their application to elucidate the phenomena of circulation.

The principles advocated in these pages afford a satisfactory solution of phenomena which are certainly inexplicable on the prevailing doctrines of physiologists. These throw no light on the circulation in the *fœtus*. It will scarcely be contended that such is under the direct control of the maternal heart. The blood flowing from the placenta does not exhibit this dependence, nor is there a direct connection between the vessels which terminate and originate in this organ,—a condition essential to the transmission of the influence. In monstrosities the heart is often wanting, and yet circulation is efficiently carried on. These phenomena present no difficulties on the views which give to the capillaries an independent source of motion. The chyle is absorbed by them, and conveyed into vessels which gradually enlarge and diminish in number, until at length they form the thoracic duct, and this cannot be ascribed to any central impulse, or the pressure of a descending column. Nor can the received views account for internal congestion or its removal; for the formation of tumours or their dissipation. To explain these effects it is necessary to have recourse to the action of the capillaries.

The experiments of Poiseuille, on the circulation of blood in the veins, are not altogether free from objection. The hæmodynamometer arrests the venous current, consequently the pressure indicated will not be the measure of the force with which the fluid is moved under ordinary circumstances, but the force resulting from the accumulation of it. The tension will in all cases be increased, but not in all situations equally. In the strictures on the application of the instrument to the arteries, this accumu-

lative force was shown to be uniform. Hence there is a striking difference in the condition of the two systems of vessels. The hæmodynamometer, as already admitted, probably furnishes an exact measure of the impulse of the heart, but the properties which enable it to solve this problem renders it altogether inapplicable as a measure of the circulation in different arteries. To whatever the instrument is applied, the course of the blood is arrested, and as it cannot possibly move forward, it becomes a small oscillating column, receiving, as a rod of iron would under similar circumstances, every successive impulse of the left ventricle. Were there any difference in the tension of arteries, according to this view, it could arise only from a difference in the length of the columns acted upon, but this could not disturb the uniformity of the results. Were the tension of the veins directly dependent on that of the arteries, though less, it would exhibit no variety. The current is interrupted by the application of the instrument, and, therefore, the tension is neither that which is natural to the individual vessel, nor to the venous system generally, but is the amount of an accumulative force, occurring in the experiment, whether made on arteries or veins. If the blood flows in the latter from the impulse of the heart, to arrest the motion of it is clearly to ascertain the force *a-tergo*. If this in one case is accumulative, it is indisputably so in every other, and yet the indications are very dissimilar in different veins. The instrument applied to the crural vein gives a pressure of 62, 55, 58, 60, 58 millimetres.

When a ligature is passed round the limb, in order to confine the circulation to the crural artery and vein, the pressure on the vein is increased to 80, 85, 75, 85, 80 mill., which is stated by Magendie to be the amount of pressure furnished

by the crural artery. How different are the effects from a similar experiment on the jugular veins! A ligature being passed round the left external jugular vein, the blood returning from the head is almost wholly confined to the right, to which the instrument is applied. The internal jugular veins in the dog are exceedingly small, and cannot modify materially the results. The mercury, however, rose only to 24, 23, 20, 23, 25, 24 mill. How remarkable is the difference in the pressure of the two veins, under very analogous circumstances, and yet the same amount of propulsive power is stated to urge the contents of both.

The reason which Arnott assigns for the difference in the tension of the arterial and venous systems, viz., the means of escape for the venous blood into the right auricle, does not explain the discrepancies in the tension of the veins, because in all these experiments the contents of these vessels are equally arrested by the instrument, and, therefore, if the impulsive force acting behind is one and the same for the whole of the venous system, the indications of pressure should be the same.

The experiment on the crural vein establishes the justness of this reasoning. In what condition is the blood placed in this vessel on the application of the instrument? Is it allowed to pursue its course from the impulse received from the heart? It clearly cannot move beyond the space arising from the slight elevation of the mercury. During the experiment, blood continues to be conveyed to the limb; consequently the artery, the vein, and the intermediate vessels become congested. This effect is inevitable, and the blood, having no means of escape, forms one unyielding column from the artery to the insertion of the instrument. The impulse of the left ventricle may possibly,

on this occasion, be transmitted to the vein, as the stroke of the piston would be communicated in a series of inert tubes. The two cases are analogous, and both exhibit a wide departure from the laws regulating vital operations.

The tension of the veins cannot be accurately determined by the hæmodynamometer. Admitting, however, that the force with which the blood circulates in them, is derived from the capillaries, the results are invaluable. According to this view, each organ generates its own propulsive power; and consequently, on applying the instrument, the accumulative force in the vessel arising from it is determined, and thus it is easy to explain the variety of tension throughout the venous system.

In evidence of the little attention which Magendie had given to the condition of the circulation, produced by the application of the instrument, other facts may be stated. He endeavoured to ascertain the changes in the pressure occasioned by the injection of water into the sanguiferous system. The hæmodynamometer is previously fixed in the crural artery, and indicates a tension varying from 60 to 110 mill. When the dog became calm, a hundred grains of water, 39° Fahrenheit, are injected, which is repeated five times without any alteration being observed in the rise of the mercury. Between the sixth and the tenth injection there is a slight elevation. The experiment was continued until about a quart of water had been injected. At this period, the physiologist wished to observe the effects of the injection of water at 113° Fahrenheit, on the same animal. The mercury was at this time oscillating between 60 and 115 mill.; after three injections it fell to,—55, 70, 65, 80, 50, 55 mill.

After the fourth injection it rose to,—80, 90, 85, 95 millimetres.

The fifth, sixth, and seventh, caused it to rise to,—100, 120, 115, 140, 125, 145 mill.

These successive injections brought the animal to the point of death. At the ninth, the mercury fell to 30 mill.; after the tenth it stood at 25, when the heart ceased to act. Even after death the mercury oscillated between,—17, 16, 15, 11 mill.*

The continuance of this pressure Magendie attributed to the fulness of the vessels.

The analysis of these experiments will not be altogether unprofitable. It will show, and the truth is important to enforce, that the achievements of the hand, in physiological investigations, leave much to be accomplished by the head. From these researches no positive conclusions can be drawn. Were the action of the heart undisturbed by the injections, it would then be easy to determine the modifications produced in the tension of the vessels, by the addition of fluid at different degrees of temperature, but certainly not otherwise. The mental sufferings of the animal are great, all of which will be accompanied by corresponding changes in the contractions of the heart, and consequently in the tension of the arteries. The amount or acuteness of the sufferings cannot be measured by the quantity or temperature of the fluid injected. Animals of the same kind will not be affected in the same manner, nor, indeed, the same animal at different times. To these circumstances, the physiologist never once alludes, but at the conclusion of his experiments, remarks :—

“ Pour avoir des renseignements exacts sur l'influence du froid et du chaud, relativement à la force avec laquelle

* Leçons sur les Phénomènes Physiques de la vie. Tome iii. pp. 236, 237.

le sang presse ses parois, il faut ne tenir compte que des premières injections. Ainsi quand nous avons vu la pression augmenter vers la fin de l'expérience, nous avons attribué cet effet non à la température du liquide, mais à son volume. Près de deux litres d'eau distillée étaient passés dans le torrent circulatoire."*

When the pressure increases towards the end of the experiment, he states, it is not referable to the temperature of the fluid injected, but to its volume. The mere increase of volume does not explain the result. When the quantity received by the veins is small, no particular disturbance is caused in the vital functions. They continue almost in their normal condition, but when the quantity is large, there is inevitably great derangement. The mental sufferings of the animal are increased, and difficulties are opposed to the free action of the heart. These circumstances tend to excite the vigorous efforts of the system. The heart must either remove the difficulties or cease to act. The struggling attempts to overcome them are the occasion of the augmented pressure. A further increase in the quantity injected at length oppresses the heart, and, as a necessary consequence, the mercury gradually falls, until it stands at 25 mill. The effect arising from the simple addition of volume does not admit of calculation.

* Tome iii. p. 239.

BOOK VI.
THE CIRCULATION IN THE ACARDIAC FŒTUS.

Considerable attention has of late years been given to the anatomy of the fœtus, and several important facts have been established that were previously not known, or only imperfectly understood. The greater exactness of knowledge attained respecting the structure of certain important organs has not, however, thrown any additional light on the functions of them. They are still involved in much obscurity. Indeed, whether the inquiry be the nature of the connection between the fœtus and the mother,—the office of the placenta,—the relations of the umbilical vein and the umbilical arteries in the substance of this organ,—the manner in which fœtal circulation is accomplished,—the uses of the amniotic fluid, or several other points of interest, a remarkable discrepancy of opinion prevails. On all these questions the highest authorities are at variance.

The attention has been drawn to the circulation in the acardiac fœtus, by an article which has just appeared from the pen of a well known writer,* in which it is attempted

* Marshall Hall. M.D. London and Edinburgh Monthly Journal, No. xxx., p. 541.

to be shown, that the circulation in the monster is effected by the heart of the perfect twin-fœtus, which, it is asserted, uniformly co-exists with it. In support of this doctrine, several facts are adduced, which, studied in the light in which he regards them, seem to afford demonstrative evidence of its soundness. Philosophically examined, they may be shown to arise from a cause very different from what is assigned, nor can they in any degree be employed in corroboration of his peculiar views. The phenomena which he attempts to trace to the action of the fœtal heart, and on which his reasoning principally rests, so far from having such origin, may be proved to be physical impossibilities. It is difficult to imagine how so enterprising and intelligent an inquirer, and one, indeed, who has devoted a large portion of his time to analytical and experimental researches, should have failed to exercise a sounder judgment in the appreciation of the phenomena which fall under observation.

A philosopher, to whom science is greatly indebted for many valuable discoveries, remarks, in reference to this subject:—

“It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries:—in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual, and with the assistance of a spontaneous dilatation; the resistance in the extreme vessels being also probably much smaller than usual; if the

motion was continued; it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap."*

This acute writer does not appear to have given especial attention to the circulation of blood in the fœtus. He contends, and justly, that the blood must have circulated as under ordinary circumstances in the veins and arteries. The rest of his observations are only ingenious conjectures. Lobstein, in his elaborate work on the nutrition of the fœtus, offers the following explanation. The passage is valuable, as affording a striking illustration of the tendency of the mind to make nature conform to our views, rather than doubt the justness of preconceived conclusions.

L'exemple des fœtus dépourvus de cœur, et qui cependant ont pris de l'accroissement, ne prouve nullement que la circulation de l'enfant se continue avec celle de la mère ; car dans les cas où le cœur a véritablement manqué, l'on a toujours trouvé quelque chose qui pouvoit en faire les fonctions. Ainsi on a souvent rencontré un épaississement, un renflement, ou un bulbe à l'artère aorte, qui remplaçoit le cœur ; et d'ailleurs il étoit possible que les gros troncs vasculaires fussent doués d'une contractilité suffisante pour suppléer à l'absence de cet organe.†

That, in the absence of the heart, the blood is propelled by any modification in the form of the aorta, is entirely a gratuitous assumption. The modification is not constant, and there is no evidence that any such condition in the higher classes of animals, is accompanied with the power of originating motion.

* Essai sur la Nutrition du Fœtus, par J. F. Lobstein, p. 73.

† An Introduction to Medical Literature, by Thomas Young, M.D., F.R.S., p. 621.

Another view, which is entertained by some writers, was proposed by Sir Astley Cooper. He imagined the circulation to be inverted, the blood entering the fœtus by the umbilical arteries and returning by the umbilical vein; and, by the further supposition that an anastomosis exists between the umbilical arteries of the perfect and imperfectly organized fœtus, he endeavoured to show that the heart of the former would maintain the circulation in the system of the latter. He remarks :—

“ In considering the mode in which the imperfect production may have derived its support, the chief difficulty is to explain how its circulation could be maintained without the force of the heart's action, as that organ was totally absent. This effect has been supposed to arise from the muscular efforts of the vessels themselves. But the difficulty is at once removed, by observing that the imperfect child was a mere appendage to that which was perfect; that there was only one placenta; and the blood which traversed its structure was derived from the perfect fœtus, and returned to it again; that the imperfect fœtus received, through its own umbilical artery, from that of the perfect fœtus, the current, which, after entering the aorta, and thence circulating through the arteries and veins of the monster, was conveyed back again by the umbilical vein of the imperfect, into that of the perfect fœtus; and that the heart of the developed child impelled the blood into the other, which was appended to it, in the same way as it caused that fluid to circulate through the vessels of one of its limbs.”*

It has been argued, in opposition to this view, that “ it is plain from the injected placenta, or double placentæ, that the umbilical artery is distributed to both portions of this

* Guy's Hospital Reports, vol. i. p. 236.

placenta. The force by which it is impelled must therefore be extended, through the capillaries, to the umbilical vein of the imperfect, as well as the perfect fœtus. If the force of the heart is directed, at the same time, into the umbilical artery, it is plain that, being balanced in the two orders of vessels, it will propel the blood along neither ; unless some other principle be introduced, to modify this force and direct its influence.”*

There are serious objections to this view in explanation of the continuance of circulation in the acardiac fœtus. The blood which circulates, under ordinary circumstances, in the umbilical vein, is not, in quality, the same as that which returns to the placenta along the umbilical arteries. In the former, it is arterial in colour, and possesses elements essential to support vital action. According to the doctrine of the distinguished surgeon, the blood which has been deprived of these elements, in the system of the perfect fœtus, is alone that which is transmitted to the monster.

This view has not the merit of being suggested by any elaborate or comprehensive considerations of the phenomena of fœtal life. It originates entirely in the imagined difficulty of accounting for the circulation where the heart is absent, assuming, what is certainly destitute of proof, that this organ is necessary to the production of the effect. The motion of the blood, in one class of vessels, is unquestionably facilitated by it in the fœtus ; but there is no evidence that the course of the vital fluid along the umbilical vein depends upon any such central power. It will subsequently be shown that this is to be traced to the action of the placental vessels ; consequently if the blood

* Dr. Marshall Hall, London and Edinburgh Monthly Journal, No. xxx. p. 543.

can flow into the acardiac fœtus without the direct co-operation of the maternal or twin-fœtal heart, it is not extraordinary that similar vessels in the monster should maintain the circulation, in a degree commensurate with its wants.

Another view has recently been brought under notice by Dr. Houston. He contends that the blood pursues its course to the imperfect fœtus by the umbilical vein, and is conveyed from it, as in the natural state of things, by the umbilical arteries, but that the circulation of it in the fœtus itself is inverted. The following are his remarks :—

“I am of opinion that in monsters of this nature, the placental circulation is normal, viz., that the blood enters the organ by the arteries, and returns by the veins, equally in the imperfect as in the perfect placenta; that there may be, and I suppose there always is, in such cases, a communication between the vessels of the two, which may account for the diminutive size of the one organ as compared with that of the other; but that the presence of such communication is more in favour of the hypothesis, that the currents take the same course in both cords, than of that which infers that they run in opposite directions.

“As to the circulation in the body of the monster, the facts already stated lead me to conclude that the course of the blood in the veins and arteries is abnormal, viz., that the fluid enters by the veins, and returns by the arteries.”*

I come, lastly, to the examination of the views of Dr Marshall Hall, who has, for a series of years, given particular attention to the subject of circulation. He attempts to disprove the foregoing doctrines, regarding the circulation in the acardiac fœtus as depending entirely on the heart of the twin fœtus. The following are his observations :—

* Dublin Journal of Medical Science, Vol. x. p. 214, 215.

“ That the action of the heart does extend to the umbilical cord of a second fœtus, is, however, proved by a different kind of fact; the blood of a retained fœtus has been actually seen to flow from the divided cord of the one first born.”*

In confirmation of this, he mentions a case which fell under the observation of M. Lallemand, the importance of which induces me to give it at considerable length.

Après un travail de quelques heures il sortit naturellement un fœtus bienportant, qui paraissait avoir sept ou huit mois. Quand on eut coupé le cordon ombilical, et lié le bout qui tenait à l'enfant, M. Patissier, qui tenait celui qui répond au placenta, s'aperçut qu'il donnait plus de sang que de coutume; ce qui fit examiner la chose de plus près. Alors tous ceux qui étaient présents purent se convaincre que le sang qui sortait était lancé par saécade à une assez grande distance, absolument comme le ferait, dans une amputation, une artère d'un petit calibre. Quelle pouvait en être la source? Le sang ne pouvait venir de la mère avec cette impétuosité et ces jets interrompus qui annonçaient l'influence du cœur: d'ailleurs, quand le fœtus est sorti, la circulation cesse ordinairement dans le placenta. Nous pensâmes donc aussitôt qu'il existait un second fœtus dans la matrice, surtout en nous rappelant la forme qu'elle avait au commencement du travail; et le toucher confirma cette presumption. Comme le jet de sang était considerable chaque fois qu'on cessait de comprimer le cordon, il fut lié. Les contractions de la matrice devenant plus fortes et plus rapprochées, l'enfant se présentant bien, l'accouchement se termina naturellement. Le second fœtus était semblable au premier. Après la section du cordon, il ne sortit pas de sang par

* London and Edinburgh Monthly Journal, No. xxx. p. 542.

le bout qui tenait au placenta : la délivrance n'offrit rien de particulier. Les deux placenta étaient réunis en une masse commune, quoique les membranes adossées ne fussent que contiguës. L'un des cordons s'implantait au centre de la masse, et l'autre sur la circonférence. On n'essaya pas d'injecter le placenta, parce qu'une portion avait été déchirée ; mais il était évident que non seulement il existait pendant la vie des deux fœtus une communication de l'un avec l'autre, mais encore qu'elle avait lieu par de gros vaisseaux, puisque le sang sortait du cordon, ombilical coupé, comme s'il n'eût été qu'une continuation d'autre. Cela peut encore donner une idée de la force de contraction du cœur chez un fœtus de sept mois environ.*

Dr. Hall remarks, "My view of the subject is this: I suppose the heart of the perfect fœtus to impel the blood of the imperfect fœtus, as Dr. Young suggested, but not immediately from its umbilical artery into that of the other, according to the view of Sir Astley Cooper, but through the capillaries of the placenta into the umbilical vein and a series of capillaries to the aorta ; and not, according to the views of Dr. Houston, in an inverted direction."†

It would naturally be inferred, from the concluding remark, that a serious difference of opinion existed between Dr. Hall and Dr. Houston in regard to the circulation of blood *in* the body of the imperfect fœtus. The former objects to the views of the latter on the ground, that he advocates an inversion of the circulation ; in other words,

* Observations Pathologiques propres à éclairer plusieurs Points de Physiologie, Par F. Lallemand, p. 35, 36, 37.

† London and Edinburgh Monthly Journal of Medical Science, No. xxx p. 544, 545.

contents that the blood passes from the umbilical vein into venous capillaries, and is afterwards collected by arterial capillaries and conveyed into the aorta. Dr. Hall is compelled to adopt the same doctrine, and, strange enough, it is implied in his own words, as far as any precise meaning can be attached to them. His words are, "I suppose the heart of the perfect fœtus to propel the blood to the imperfect fœtus, through the capillaries of the placenta into the umbilical vein, and through the umbilical vein and a series of capillaries to the aorta." The connection between the aorta and the umbilical vein, in the æcardiac fœtus, must depend on capillaries, and which are performing an abnormal function, at least in degree, compared with their office after birth; but in such a case of monstrosity it cannot possibly be otherwise. The absence of a central organ to receive and propel the blood, through two different orders of vessels, renders this condition inevitable. How is the blood to get into the umbilical arteries, in its return to the placenta, except by passing through the mass of capillaries which connect the umbilical vein and the aorta? The views of Dr. Houston are no more an inversion than those of Dr. Hall, nor is there the slightest apparent difference in their views concerning the circulation of the blood *in* the fœtus. Nature seems scarcely to have left room for a difference on this particular point.

With the absence of the heart, the ordinary connecting links between the venous and arterial systems are destroyed. The *ductus venosus*, the *ductus arteriosus*, and the *foramen ovale*, which, in the fully organized fœtus, establish peculiar and temporary relations to the aortic system, are wanting; and consequently, unless the two former exist, the aorta must derive its blood from some other channels; and through what other channels, except "a series of

capillaries," as stated distinctly by Dr. Hall himself? It is, indeed, singular that he should object to the views of Dr. Houston, on the ground that they are founded on an inversion of the circulation, when his own, as far as they are made known, present no difference.

If large vessels were discovered to join the umbilical vein with the aorta, by which a large portion of the contents of the former were transmitted to the latter, this would not essentially modify the question of circulation in the acardiac fœtus. The physiologist would still have to explain how the blood moves throughout its entire system without the aid of a central propelling organ.

The doctrines of Dr. Hall, to be studied with advantage, must be treated under several different heads. It is necessary first to examine the interesting case related by M. Lallemand, as all that is plausible in the reasoning of both physiologists is derived from the consideration of it. It is also important to observe, that the phenomena in this case are not imagined to be evolved by any departure from the ordinary laws of organization; in other words, that they are not peculiar, but universal in all cases of twins. The flowing of the blood in jerks from the umbilical cord of the child first born, is considered by Lallemand, as well as by Dr. Hall, as indisputable evidence of the direct influence of the heart of the second fœtus. The two physiologists differ however in one essential respect in their views. They both equally ascribe to the heart the ejection of the blood in jerks; but the former, and by this means he gets quit of one insuperable objection, states that the phenomenon is not only evidence of a communication, during fœtal life, between the twins, "but further, that this was established by large vessels, since the blood issued from the cut umbilical cord as if it had been only a continuation of the

other. This may communicate some idea of the force of the contraction of the heart in a fœtus of about seven months."*

Dr. Hall does not explain the phenomenon on the supposition of an immediate communication between the principal vessels of the twins, but contends that the blood traverses the capillaries of the placenta in its passage to the umbilical vein of the first born fœtus ; and thus, as will hereafter be shown, he adds another difficulty to the one involved in the doctrine, which teaches the direct action of the heart on the motion of the blood in the cord of the twin fœtus. The first step is to analyze this doctrine in its general bearings, and endeavour to point out some of the strange conclusions to which it obviously leads.

Whether the ejection of the blood from the umbilical vessels be examined according to the doctrines of either physiologist, it is obvious, from the force with which it is emitted, that the communication between these vessels in the divided cord, and the heart of the fœtus *in utero*, is most extensive. The blood that flows along the cord of the child first born must have been transmitted in this course previously to its birth. These authorities state that the communication is natural, and the escape of the vital fluid in a strong current is adduced as evidence of the fact. If such are the relations between the cords after birth, is it not evident that, during foetal life, each heart is engaged in propelling blood, not only towards its own placenta, and, as might be imagined, facilitating the return of it in the umbilical vein back into the fœtus, but in urging the blood along the umbilical vein of the accompanying fœtus ? If this is the direction of the blood after the birth of the

Observations pathologiques propres à éclairer plusieurs Points de physiologie, p. 37..

first child, it must have been equally so previously. It would be unphilosophical to argue for the one and not to allow the other. According to M. Lallemand, the blood flows from the cord as from a divided artery, in case of amputation; and as the impulse of the heart, supposed to be thus exerted, is neither new nor aggravated by any circumstance, its influence must have been equally extensive in every stage of foetal life.

It is not easy to see how this objection can be obviated. The doctrine contends for a communication between the two cords, as free as between any artery and vein in the adult body, and a communication that must be as open previous to, as after the birth of either twin. This circumstance is alone sufficient to awaken suspicion of the correctness of the explanation; but it is associated with another difficulty. The blood that escapes from the divided cord is a continuous column with that which is moving in the body of the retained foetus, and it is a law of the circulating system, that blood will rush towards parts where there is the least resistance. What is to prevent the blood of the foetus flowing in an impetuous current in the direction of the divided cord? Such is the phenomenon on the division of an artery, and were it to occur previously to birth, death would be the result. A continuous column is implied in the doctrine; if not, how can the heart of the foetus expel the blood from the divided cord of the born child? Without this condition it is an impossibility; and with it, what is to prevent the blood rushing towards the placenta and the divided cord? In whatever light the doctrine is examined, it is fraught with difficulties. This extensive influence of the heart of the retained foetus rests on no clearly esta-

blished facts; nor is it required to account satisfactorily for the character of the stream emitted from the separated funis.

Had not Dr. Hall adduced the case of M. Lallemand, in confirmation of his views, the communication between the cords of the perfect and imperfect fœtus might have been imagined as peculiar—as an adaptation to meet extraordinary exigencies. But, so far from looking upon it as irregular and characteristic of monstrosity, he regards it as existing in every case of twins, and thereby involves himself in inextricable difficulties. The division of the cord is, therefore, only the occasion which exhibits the communication, and not the occasion which establishes it for the first time.

M. Lallemand, sensible, it would appear, of the difficulty of accounting for the jerks in the ejected stream, had the blood, previously to reaching the cord, circulated through a mass of capillaries, avoids what would have been an additional difficulty annexed to the doctrine, by stating that the communication “*avoit lieu par de gros vaisseaux.*” It is scarcely necessary to remark, that a communication between such vessels is a gratuitous assumption.

Agreeably to the views of Dr. Hall, it is not by the connexion of large vessels that the heart of the one twin transmits an impulse along the cord of the other. He contends that this influence passes through the mass of capillaries between the two cords. “The difficulty,” he observes, “is to conceive that this influence can be so extended through the placental capillaries. A further difficulty would be to conceive that the same influence can extend through a second series of capillaries in the body of the imperfect fœtus, for this is essential to the return of its blood to its placenta,—in a word, to the circulation of

the blood."* For the present we will leave the analysis of the circulation in the imperfect fœtus, in order to examine his views respecting the transmission of the blood through the placental capillaries to the cord of the first born child.

There is no instance, in the vast range of animal life, where the blood, issuing from a mass of capillaries and terminating in one large vessel, in the natural condition of circulation, exhibits, when it is allowed freely to escape, the force and jerking character of a divided artery. The phenomenon is an impossibility, and a little consideration will show the difficulties with which such a doctrine is fraught. It is not imagined by these writers that the umbilical vessels exert any particular influence in the projection of the blood. The effect is referred to the fœtal heart alone, and is regarded as the usual and legitimate result of such a power acting *a-tergo*.

If the heart of the fœtus causes the blood to flow in jerks from the umbilical vessels, which bear the relation of veins to it, in consequence of the mass of intervening capillaries, how does it happen that the adult heart does not propel the blood through divided veins as through divided arteries? When both classes of vessels are placed precisely in the same conditions, the blood comparatively trickles from the one, while it is ejected from the other with considerable energy, and as long as the heart continues to contract. If the heart, in the one case, impresses upon the column of blood issuing from capillaries an impulse characteristic of its direct action, the same phenomenon should be observed in the other. In no class of animals, possessing a central organ of circulation, is the motion of blood, in large vessels receiving the contents of capillaries, at all

* See Journal, *supra cit.* p. 544.

analogous to that in arteries ; nor is it ever, when permitted freely to escape, ejected with force from them.

The mass of blood in the placenta, between the vessels of the twin-cords, is many times greater than the quantity sent out by each contraction of the foetal heart ; and, according to the doctrine of this physiologist, every particle of this mass is urged in the direction of the cord of the accompanying or expelled foetus, and the current which escapes, on the division of it, is stated to exhibit the force and rapidity of arterial circulation. This is a physical impossibility. It might occur, were the blood in the umbilical vessels of the one foetus moving in continuous and corresponding channels to the vessels of the other, but not otherwise. When these vessels terminate in a mass of capillaries containing many times the quantity of blood which is at any one moment added to, or removed from them, the quantity which escapes cannot possibly exhibit the jerking character or force of that which enters.

In proportion as the mass of blood in the intervening placenta exceeds that which is added to it by each contraction of the foetal heart, in such ratio will the motion of the mass be less than that in the umbilical arteries, and also, to a certain extent, the current which issues from it. The difference in the capacity of the arterial and venous systems is a just measure of the difference in the rate of circulation in each. Were the communication between the twin-cords to be admitted, the umbilical arteries of the foetus *in utero* would necessarily have the relation of arteries to the propelling power, and the divided umbilical vessels of the child born, that of veins. It would be impossible for the latter, having only the capacity of the former, to remove, in equal times, the quantity of blood conveyed to the placenta by the contractions of the heart.

The blood ejected would have a rate of motion corresponding with that of the mass between the two cords, and not with that receiving the direct impulse of the heart and conveyed into the placenta.

Had Dr. Hall imagined the communication between the umbilical vessels of the twins to be formed by large and obvious anastomoses, and not in the body of the placenta, by capillaries, the supposition that the influence of the foetal heart *in utero* extended to the vessels of the divided cord, if not tenable, it would not have been open to the same objection as his own. That in fact, derives its plausibility from phenomena, which cannot occur on the principles which he lays down. A strong and jerking current, exhibiting the direct impulse of the heart of the retained foetus, cannot be observed in divided vessels, separated from this organ by a mass of capillaries. He admits* that the motion of the blood in the capillaries and veins is rendered uniform by a peculiar anatomy, and yet, in treating of the circulation in the acardiac foetus, he argues for this jerking current, when every particle of the blood of which it is formed, according to his own ideas, has had to pass through a mass of capillaries to reach the divided vessels of the funis.

According to his doctrine, the impulse of the heart of the perfect foetus, transmitted through its umbilical arteries, not only propels the blood in the umbilical vein of the imperfect foetus, but, by what is called lateral action, draws the blood in its umbilical arteries towards and into the placenta. "Thus, then, in reality, and on two different principles, the blood is propelled to, and attracted from, the imperfect foetus, by the power and action of the heart

* See his work, *supra cit.*, p. 26.

of the perfect one, and along the customary channels."* Thus, conjecture is added to conjecture—hypothesis to hypothesis, and conclusion to conclusion, until the whole breaks down, from the superstructure having shot far beyond its narrow foundation. Of the influence of such lateral action there is no evidence, nor does he possess any knowledge whatever of the nature of the relations between the umbilical arterics of the perfect and those of the imperfect fœtus, so as to show how the blood in the latter vessels is drawn towards and into the placenta by the current flowing in the former. It is greatly to be regretted that a sounder and more cautious spirit is not exercised in the cultivation of physiology. This method of imagining what the mind does not perceive, and endeavouring to prove how nature acts, when her operations do not fall under observation, gives rise to many ingenious speculations, but to few truths of extensive application.

If any apology were necessary for these lengthened remarks, it would be found in the importance of the subject,—in the respectability of the writers whose opinions are analyzed, but especially in the interest which all must feel in the establishment of just principles and correct reasoning. The inquiry is not only valuable in reference to fœtal life, but equally so in regard to vital operations generally. It is scarcely possible to arrive at any fact elucidating the conditions of the former, that does not at the same time throw light upon the functions of the latter.

In examining the views of distinguished writers, it has been shown that the admission of them leads to conclusions which, if not incompatible with fœtal existence, is attended

* London and Edinburgh Monthly Journal, No. xxx. note, p. 545

with serious difficulties ; and, further, that the ejection of the blood from the divided cord,—which is the principal circumstance on which the doctrines of these writers rest, cannot possibly arise from the action of the fœtal heart. It remains now to explain the cause of the phenomenon, which, indeed, occasionally occurs.

It is remarked by M. Lallemand that the jerking character of the current cannot be ascribed to maternal influence, because on the birth of the fœtus, where there is only one, it is not observed. This is perfectly just ; and, moreover, the circumstance may generally be regarded as evidence of the existence of a second fœtus. Without entering minutely into the consideration of the structure of the placenta, there are a few observations respecting the disposition of its vessels to which it is important to direct attention. This organ is composed chiefly of capillaries, but these have very different relations to the arteries and vein of the cord, and which it is essential to point out. The arteries may be viewed as terminating in the placenta, dividing and subdividing until they are lost in its complicated vascular structure ; and the blood which they convey is urged forward by a force *a-tergo*. When the cord of the expelled fœtus is divided this force is interrupted.

The umbilical vein originates in the placenta, in capillaries, which gradually increase in size from within outwards, until at length they form the vein of the cord. The circulation in it is to be traced to some power in the placenta. To avoid, in this stage of the inquiry, the expression of any decided opinion on this subject, it may be simply stated, that the influence by which the blood is moved in the umbilical arteries acts in the direction of the placenta,—that which promotes the circulation in the vein, in the direction of the fœtus. Therefore, were pressure

exerted on the placenta, it seems probable that the blood would escape more readily from the vein than from the arteries. In the former, to flow *out* is its natural course; in the latter, to flow out is its inverted action.

After these remarks, the general accuracy of which is unquestionable, it is easy to explain the force and jerking character of the current on the division of the funis. The impulse with which the blood is ejected, IS TO BE TRACED ENTIRELY TO THE CONTRACTIONS OF THE UTERUS, by which the blood is pressed out in the direction of the vessels of the divided cord. It is almost impossible for such contractions to take place, and not to produce, in some degree, this effect, at least occasionally. The powerful efforts which the uterus makes to expel the second fœtus, and to regain a diminished capacity, is a pressure which is exerted on the mass of the placental vessels; hence the blood will, at times, escape from the divided cord, in jerks, in force and frequency according to the severity and number of these contractions. Why the phenomenon should occur only in cases of twins, is not difficult of explanation. There are several interesting circumstances connected with the condition of the uterus at this time, which do not exist with one fœtus, and which are worthy of particular attention.

1. It is remarked by M. Lallemand, that in cases of single birth, “when the fœtus is expelled, the circulation usually ceases in the placenta.” This organ is, at this time, far from being in the same condition as when the birth is double. The vitality of the placenta depends on two things,—the presence of the fœtus, and the undisturbed circulation in the uterus. A change in either circumstance at once modifies its condition. It is not at all necessary, for the broad line of argument which is

pursued, that the inquirer should be able to state distinctly the exact nature of the connection between the placenta and the uterus, or the fœtus and the placenta. The generally admitted and indisputable fact, that the placenta receives blood from the fœtus and the uterus, is amply sufficient for the purpose.

A severe disturbance in the maternal system, by which the circulation is greatly affected, as by sudden fright, or any distressing mental emotion, frequently causes abortion or miscarriage, *and is occasioned by the disorder induced in the circulation between the placenta and the uterus.* The death of the fœtus in the womb generally gives rise to the same effect, and from the same cause—disturbance of the circulation in the placenta. This organ is simply the medium of communication between the child and the uterus, and its functions cease with the destruction of this relation. When the fœtus is expelled, and the cord is divided, in case of single birth, it is manifest that the placenta can neither receive blood from the child, nor transmit any to it; hence the mass of placental vessels receiving and circulating the blood conveyed by the umbilical arteries, or urging it into the umbilical vein, in its passage to the fœtus, have no longer any office to perform. Under these circumstances the placenta becomes a foreign body in the uterus, and is quickly expelled. The circulation in it cannot be maintained by its existing connections with the uterus; consequently it would be unreasonable to expect that blood would flow, after the birth of the child, either in quantity or with considerable force from the divided cord. It does, however, occasionally escape from it, and probably from the umbilical vein, arising from the action of the capillaries in immediate relation with it, facilitated by the contractions of the uterus, while its connections with the placenta are not materially interrupted.

2. In ease of twins, after the birth of one, the placenta is not in the condition here described. The ordinary relations between the uterus and the placenta, or this organ and a foetus, are still kept up to a certain extent; and the blood, in diminished quantity, continues to flow from the uterus to the placenta, and from this to the foetus. There is, therefore, a remarkable distinction in the condition of the placenta in the two cases. In the one, circulation in it is disturbed and is soon arrested. In the other, it is in its ordinary state, as far as it is possible to be maintained by the remaining twin-foetus.

3. The continuance of the circulation in the placenta, co-existing with the second foetus, is the important circumstance to which attention is solicited. Soon after the expulsion of the first child, a certain amount of disturbance is, of course, produced in the placental circulation; and this is quickly followed by the vigorous contractions of the uterus. *These contractions are to be viewed as successive acts of pressure, both upon the foetus and the placenta,* and the blood will be forced in all directions, wherever it has an opportunity of escaping. It will not be doubted that the numerous capillaries in relation with the cord of the expelled child, will, if the circulation in them is still carried on with any degree of vigour, pour their contents in the direction of the vessels of this cord, and the blood will be ejected in force and in the frequency of successive impulses, in harmony with the energy and number of the contractions of the uterus. In cases of single birth, after the expulsion of the foetus, the contractions of the uterus are generally exceedingly slight, and, for the most part, seldom take place until the circulation in the placenta has nearly ceased; so that the umbilical cord is placed in very different circumstances, in the two cases. In the one, the

ejection of the blood in jerks and in considerable force is occasionally an inevitable effect. In the other such a result would be out of the ordinary course of nature.

Such, according to this analysis, is the cause of the phenomenon, which has been regarded as evidence of the influence of the second foetus, transmitted to the divided cord of the first born. In the views brought under consideration, have any important data been assumed, as the foundation of them that are not legitimate and obvious deductions flowing from the physiological conditions, briefly, but clearly specified? If any such objection, however, should be urged, a very slight examination will show that the charge falls with ten times greater force against others whose doctrines have been brought under consideration.

Whether the blood, which flows from the divided cord, comes from the umbilical arteries or the veins, is a point on which, perhaps, it would be rash to express a decided opinion. It might possibly be ejected from both, but it seems much more philosophical to trace it to the vein than to the arteries. The former is in intimate relation with numerous capillaries, whose office it is to furnish the vital fluid to be conveyed by this vessel into the foetus, and therefore it is reasonable to regard it as the channel through which the blood escapes.

M. Lallemand adduces another case of twins where one died in the uterus, owing to the loss of blood from the divided cord, and which was found pale and exsanguineous, and this he introduces as conclusive evidence of the justness of his views respecting the influence of the foetal heart on the umbilical vessels of the expelled child. From the importance which he and others attach to this case, in reference to the present inquiry, it is imperative to examine its merits. The following are his remarks :—

Si l'observation que j'ai rapportée ne paraissait pas concluante, en voici une autre qui semble en être le complément.

A quelques jours de là, un professeur d'accouchemens, recommandable sous tous les rapports, fit part à ses élèves du fait suivant. (Je crois devoir me dispenser de le nommer, quoiqu'il l'ait rapporté publiquement avec une franchise qu'on ne peut trop louer.) Appelé près d'une femme en travail, il reconnut, après la sortie d'un premier fœtus né vivant, qu'il en existait un second dans l'utérus. Occupé de l'enfant, il n'examina pas la portion du cordon qui tenait au placenta. Bientôt le fœtus resté dans la matrice exécuta des mouvemens brusques et comme convulsifs, que le praticien reconnut, sa main étant appliquée sur l'abdomen ; ils étaient si violens, qu'ils causaient à la mère des secousses fort douloureuses ; mais au bout d'un instant ils cessèrent tout-à-coup. La tête était alors descendue dans l'excavation du bassin : l'application du forceps paraissant indiquée, elle fut faite promptement et sans difficulté. Ce second fœtus était aussi fort, aussi, bien conformé que le premier, mais il était pâle, décoloré, tout-à-fait exsangue ; aucun secours ne put le rappeler à la vie.

This case certainly establishes two facts ;—the loss of blood from the divided cord, and the death of the fœtus in consequence. It is not, however, shown how they prove the direct influence of the fœtal heart on the blood flowing in the divided cord. The question in dispute is not the flowing of the blood, but the cause of it, which is not in any degree elucidated by this case. It illustrates the prevailing tendency, even in minds of a high order, to

* Opus supra cit, p. 37 38.

jump prematurely at conclusions, rather than analyze the phenomena which fall under observation.

The condition of the placenta, after the expulsion of one of the twins, has been explained, and it has been stated that the circulation is maintained in it with considerable vigour. The blood, which is supposed to be derived from the uterus, flows in two directions,—towards the fœtus in *utero*, and towards the divided cord. It is more than probable that the vessels of both cords are lost in the same mass of capillaries. Experiments are related in which the double placenta has been thoroughly injected through the vessels of one cord. Vessels may, however, be injected which have no necessary relations to each other as continuous channels, hence such experiments are not free from serious objection.

I do not, on this occasion, presume to express an opinion on the nature of the connection between the ultimate divisions of the vessels of the two cords. It is probable that the umbilical arteries have one termination in common; that is, in the same class of vessels; and that the umbilical veins have also a common origin.

The death of the fœtus, from loss of blood, is regarded as a fact, though only of occasional occurrence; or perhaps, in less exceptionable terms, the death of the fœtus and its exsanguineous condition are phenomena which, at times, are observed. The difficulties which involve the consideration of the subject require this guarded mode of expression. It is almost needless to remark, that the life of the fœtus *in utero* depends on the regularity of the flow of blood to and from it. To interrupt the supply to it, or to facilitate the escape from it, in a degree disproportionate to the supply, is to endanger or destroy its life. It is possible that one of those effects may take place in

conjunction with the flow of blood from the divided cord, and yet this may be only the *indirect* cause of it. It has already been stated, that sudden and severe mental emotions frequently cause miscarriages, from the derangement induced in the motion and distribution of the blood, by which the circulation in the uterus, and consequently in its temporary appendages, is disturbed. The escape of the blood from the divided cord may possibly so affect the circulation in the placenta, that the blood of the umbilical arteries of the fœtus may have a tendency to rush into the substance of it, but not out of it, through the divided cord. Or, from the current which escapes, which is probably from the umbilical vein, the blood, which ought to flow through the corresponding vein of the fœtus, may be at rest, or drawn in some other direction. In this case death would result, previously to which, however, a great portion of the vital fluid in the fœtus would have been conveyed into the placenta by the umbilical arteries. It is remarked,—

“Bientôt le fœtus resté dans la matrice exécuta des mouvemens brusques et comme convulsifs, que le praticien reconnut, sa main étant appliquée sur l’abdomen, ils étaient si violens, qu’ils causaient à la mère des secousses fort douloureuses ; mais au bout d’un instant, ils cessèrent tout-a-coup.”* These violent motions of the fœtus would take place on the disturbance of the circulation in it, but this is as easily explained on the supposition that blood has ceased to flow through the vein, as that it has flowed in too great abundance through the umbilical arteries into the placenta.

According to these conjectures, the escape of blood from the divided cord would occasion the death of the fœtus,

* Opus supra cit, p. 37 38

and this might be found to have lost the greater part of its blood, not because it had been ejected from the severed funis, but because it had accumulated in the placenta. Thus, the loss of blood from the cord would appear to account for the death and exsanguineous condition of the fœtus, when it might possibly be only the indirect cause of the result.

Let it not be urged that these are only conjectures in opposition to facts. They are offered only as conjectures, not, however, in opposition to, but in the absence of facts. The inquirer must often have felt, in the investigation of vital phenomena, the justness of the remark, that there are more false facts than false theories. It is not intended to question the two facts stated by M. Lallemand, viz., the character of the stream from the divided funis, in the case of twins; or the death and exsanguineous condition of the fœtus, in conjunction with it; and these are the only two facts with which we are favoured.

It is not possible to select, from the whole range of physiology, a more forcible illustration of gratuitous assumption than is furnished by distinguished inquirers on this subject. Their views are conjectures without elaborate consideration,—without even casting a thought on the vast field of phenomena between the effect and the imagined cause. From the jerking character of the current, to the influence of the heart of the retained fœtus, there is no investigation to show whether the blood comes from the vein or the arteries, and yet the determination is fraught with important truths. There is no effort made to ascertain whether the phenomenon is possible,—no statement of the difficulties attending every step of the inquiry. We are simply informed that the blood escapes in jerks, and with that rapidity of conception peculiar to genius, it is at once

decided that jerks are evidence of the action of a heart, and as there is no such organ in direct connection with the cord which exhibits them, the heart of the retained fœtus, which has its own funis and its own maternal sanguineous relations, is abruptly seized upon to explain the circumstance. And this is made to do extraordinary duties,—to send the blood through the greater part of one foetal system, through its own cord and placental vessels, and through a mass of capillaries, and yet, after all, to cause the blood to flow from the divided cord of another fœtus, with the same precise character that it would have been ejected from its own arteries !

Other considerations tend strongly to call in question the justness of the view entertained by M. Lallemand and Dr. Hall, respecting the action of the foetal heart on the divided cord of the first born child. The former writer observes, “ Cependant, parmi les auteurs qui ont écrit sur les accouchemens, les uns ne parlent pas de la ligature des deux bouts du cordon dans les cas de grossesse composée ; les autres la regardent comme inutile ; quelques uns seulement la recommandent vaguement, sans paraître y attacher une grande importance.”* Did the ejection of the blood from the divided funis arise from the cause assigned, viz., the heart of the retained fœtus, such a phenomenon, in place of being only an occasional occurrence, would be the rule, and its non-appearance the exception. To divide an artery, of the size of the vessels of the cord, in any part of the body, is to produce invariably a strong current in relation with the successive impulses of the heart.

The escape of the blood from the cord is regarded by these writers in the same light, and the attention is directed to it as an effect springing out of an obvious cause,—the

* *Opus supra cit*, p. 38.

contractions of the fœtal heart. Why is not the phenomenon an equally universal occurrence? If the heart, in one case, always expels the blood with force, why does it not in another? This is an important question for those who contend for a similarity in the causes. The phenomenon occurring only occasionally, is an insuperable objection to the doctrine which ascribes it to the action of the fœtal heart.

It is not, however, equally opposed to the views by which it is proposed here to account for the result. According to these, it depends on the condition of the placenta, and on the energy of the contractions of the uterus—circumstances liable to numerous modifications. I will endeavour to point out the nature of some of them. If the birth of the first twin has been easy and by no means abrupt, the circulation in the placenta in relation to this fœtus, or in that part of the placenta which is more or less common to both, may have assumed a condition preparatory to parturition. It may have become enfeebled, as well as the connection between the placenta and a certain portion of the vessels of the uterus less intimate, so that on the expulsion of the child the uterine vessels do not continue to pour into the placenta a large stream of blood, but are much in the same state as when the fœtus is expelled in cases of single birth. If, on the other hand, the labour has been sudden, or has taken place before the uterine vessels have adapted themselves to an altered state of things, the subsequent contractions of the uterus will, in all probability, cause the blood to flow freely from the divided funis. The phenomenon will depend on the condition of the placental and uterine vessels in connection with this funis.

The same gradual changes, with respect to the retained foetus, may be an additional circumstance preventing the ejection of blood, though I do not profess to understand the influence which it would exercise, except on the supposition that the mass of vessels in the placenta in relation to one cord, may anastomose with the corresponding mass in relation to the other. The admission of this offers no very forcible objection, because the diminution in the activity of the uterine circulation, to which I have alluded as an important condition, will, of course, lessen the vascularity of the placenta, and proportionately the tendency to the escape of blood.

The views which are here brought under consideration will explain why, occasionally, in cases of single birth, blood escapes freely from the funis in connexion with the placenta. It sometimes happens that the foetus is expelled before the circulation between the placental and uterine vessels has been materially diminished; consequently, the remaining activity in the capillaries of the placenta will cause the blood to flow from the divided cord. The severe contractions of the uterus not only tend to expel the child, but, in an equal degree, to lessen the activity of the uterine circulation, and, according as this is effected, will the subsequent condition of the placenta be modified.

I pass now to the analysis of some other views which have been brought into prominent notice by Dr. Hall, and imagined by him to confirm the justness of his arguments, with respect to the circulation in the acardiac foetus. After adverting to the case of M. Lallemand, he endeavours to show that there is no objection to his own doctrines, on the ground of the extensive influence which he assigns to the heart of the retained foetus. As already remarked, this influence is conceived to be transmitted, not

only through the capillaries of the placenta, but, when the acardiac fœtus is one of the twins, through the entire system of the monster. "But have we not," he says, "examples of such extended influence of the heart? Do we not observe that, in fishes, the blood is distributed first through the capillaries of the gills, then through the capillaries of the body, and, lastly, again through the capillaries of the portal circulation? If any one doubt the extent of this power, it is incumbent on him to show that there is indeed an auxiliary or additional one; and of this I again assert, we have hitherto no proof."

If the inquirer into physical science were allowed to assume his facts, as the physiologist does in this instance, the study of philosophy would call largely into exercise the imaginative powers of the mind. So far from the circulation in fishes being evidence of the justness of his reasoning, it might be adduced in support of the co-operation of the capillaries in the process of circulation; and there would be much less assumption in this than in the use made of it by the writer. In fishes the heart sends the blood to the gills, as the right ventricle in warm-blooded animals transmits it to the lungs; and in both cases, for the same purpose,—to acquire vital properties. In the latter instance, it is conveyed into the left ventricle, by the contractions of which it is sent throughout the arterial system. In fishes there is no such corresponding cavity to receive and propel the vital fluid after the chemical changes in the gills; and from this circumstance, in conjunction with numerous other considerations, it is reasonable to infer the influence of the capillaries in the process of circulation. Why the phenomena should be imagined to confirm the views of Dr. Hall, and should be seized upon as setting at rest the question

in dispute, viz., the extent of the influence exercised by the heart, it is difficult to understand.

In some experiments, which he relates, on the eel, he attempts to prove that the agency of the heart is transmitted through a secondary series of capillaries. "We placed, he observes, "the pectoral fin of the eel in the field of the microscope, and compressed it by the weight of a heavy probe. The movements of the blood became most obviously pulsatory, pretty regularly about twenty times in the minute—the precise number of the pulsations of the heart. The heart's action extends, therefore, through the branchial capillary vessels, to the secondary series of capillaries in the body of the animal. We now tied the vessels proceeding from the heart; all circulation in the tail and fins subsided almost immediately, affording a proof that the capillaries have in reality no power of sustaining the circulation. Now, if in any animal the capillary vessels have a power of their own, it is surely in the eel. For this is the longest of animals, and its systematic circulation is entirely secondary, both circumstances requiring such power."

Having, in the previous pages, exposed the unphilosophical character of such experiments, and the sources of fallacy to which they are open, few remarks on this occasion will be necessary. Had the mind been unacquainted with this branch of physiology, these experiments would have led one to suppose, that, antecedent to his labours, the circulation of blood from arteries through interminable capillaries into veins, had never been clearly demonstrated. Indeed, the only value which they possess is to prove that blood flows from one class of vessels into another. There is, however, one novelty in the conception of them, and that is to show

that the capillaries exercise no influence on the motion of blood, and to establish this he deprives them of the vital stream,—a condition essential to their action. The first experiment consists in placing a heavy probe on the pectoral fin of the cel, for the purpose of arresting the course of the blood at this point, so that between the probe and the heart there would be a series of columns of blood at rest, the vessels in which they are contained assuming the condition of inert tubes. As the blood cannot possibly move forward, it is evident that every additional quantity sent into these vessels by the contractions of the heart, will aggravate the existing tension, and at length this organ acts on these columns, as the piston would act upon a series of inorganic tubes. The writer remarks, that he perceives, in the secondary series of capillaries, pulsatory movements, synchronous with the contractions of the heart. It would be strange if he did not. What is the blood to do that is constantly added to the columns by the successive contractions of the heart? The probe prevents it moving forward, so that every subsequent quantity must return into the heart, or impress upon these columns an impulse which must be perceptible throughout the whole extent of them. If such impulse were not perceptible, it would be because there was no continuous channel between the probe and the heart, the channel existing, the pulsatory movement is inevitable. The phenomenon, to which attention is directed, might have been calculated upon independently of all experiments. I admit the pulsatory movement, and confess I should have been more astonished at its absence than I am at its presence.

His next experiment is to show, that the capillaries are entirely destitute of all propulsive power, and this he proves

in a very peculiar manner. The reader would imagine that in order to determine this important question, the physiologist would give the vessels fair play. Is it common justice to the capillaries, in the investigation of their functions, to tie the vessels immediately proceeding from the heart, after which the capillaries cannot receive a particle of blood, and then inquire whether they have the power of urging this fluid forward? Such is the experiment to which attention is solicited, and which is deemed to be as conclusive in its results as it is original in its conception.

BOOK VII.

THE MORBID PHENOMENA OF ARTERIAL AND VENOUS ACTION.

To obviate objections to some of the arguments enforced in the preceding pages, it is necessary to advert to certain morbid conditions of the circulating system, brought forward by writers, in corroboration of the independent action of particular arteries; and, also, of the transmission of the impulse of the heart to the last divisions of the veins. Laennec, in treating of the increased pulsation of the arteries, remarks :—"This phenomenon is the best proof that the arteries have an action of their own, independent of that of the heart. It is thus by no means very rare to find the pulsation of one of the carotid or temporal arteries vastly greater than that of the other. A like difference is still more common in the radial arteries; it even exists in the state of health, in most men, the right pulse being almost always stronger than the left. I have sometimes observed, during the course of a disease, the radial arteries become alternately stronger and weaker; or the left become the stronger of the two, although the contrary has been the

case in health. This morbid degree of impulse is not at all unusual in the aorta, particularly in the central portion of it. A sense of fulness always attends this augmentation of impulse, *the affected artery seeming to be always as full as possible*, and more than the other parts of the arterial system."*

The consideration of this subject is not unimportant in a practical point of view. A knowledge of the nature of the morbid phenomena, if full and accurate, will lead to juster views of disease, and to the suggestion of sounder principles of treatment. The increased pulsation of the arteries is an unquestionable fact. It does not, however, in my opinion, arise from any particular change in the action of the arteries themselves, but is the result of causes to which the physiologist has given no attention.

The fulness of an artery, and the resistance which it offers to the finger, do not, as is generally imagined, depend entirely on the quantity of blood which is transmitted by the left ventricle. There is another important element to be taken into account, viz., *the more or less freedom with which the blood can pass from arteries into capillaries*, which has been altogether overlooked. The quantity and force with which the fluid is sent from the heart will produce, in all arteries of the same calibre, an uniformity of tension; and were they the only agents influencing the character of the current, an unnatural or aggravated pulsation of a particular artery would not occur. The same causes will give rise to the same general effects. An apparent exception to the rule will always be traced to the operation of some condition which does not ordinarily exist.

The aggravated pulsation of arteries is most frequently observed in the carotid and temporal, and from very obvious

* *Traité de l'Auscultation*. Translated by Dr. Forbes.

reasons. The brain receives a larger proportion of blood than any other organ of the body. It is in fact one immense mass of capillaries, and from the important and diversified functions which it performs, the greater is the liability of these vessels to temporary changes. They are not only subject, in common with the capillaries generally, to the influences of constitutional derangements, but especially to all the variety of mental and nervous diseases.

It is not in the power of the physiologist to state the precise modifications in the condition of the brain, giving rise to an excited action of the carotid or temporal arteries. That there is some impediment to the circulation is evident from the fulness of the arteries, which fall under observation. If the blood does not flow in them with its ordinary facility, it is clear that an increased amount will remain to be acted upon by the impulse of the heart, so that the same impulse will produce different effects in arteries, according to the ease with which the blood escapes into the capillaries. Hence the undue pulsation of the carotid or temporal artery is not occasioned by the independent and excited action of the vessel, as is supposed, but by the presence of an increased column of blood against which the force of the heart is directed. It is admitted by Laennec, that "a sense of fulness attends this augmentation of impulse, the affected artery seeming to be always as full as possible, and more than the other parts of the arterial system." This is strictly in confirmation of the explanation which is here given. The fulness of the artery proves that the blood passes with difficulty into the capillaries; and, therefore, the ventricular impulse transmitted to the augmented arterial column, will cause a strong and obvious pulsation, as if it were the result of the excited action of the vessel. The same phenomenon is

observed in arteries leading to an inflamed part. They become enlarged, and appear to act with increased energy. In all these cases the local pulsation is evidence of obstacles to the circulation, and not of the independent action of the vessel.

The strong beating of the ventral portion of the aorta may arise from two different causes, either from obstruction in the capillaries of the abdominal viscera, or from pressure upon some part of the aorta. Whichever view is adopted, the explanation is the same. The contents of the aorta have not their usual facility of escape, and consequently the impulse of the heart, meeting with more than ordinary resistance, causes in some part of the column a sense of pulsation.

The excited action of particular arteries frequently co-exists with great irritability. The individual is anxious and morbidly sensitive; hence it is that the phenomenon is often regarded as nervous in its character and treated accordingly. The disorder of the nervous system offers no objection to the foregoing arguments. In conjunction with other symptoms, it is evidence of an exhausted or enfeebled condition of the vital powers. The secretions are vitiated and the circulation is weak. The practice which is often successfully pursued in such cases, is change of air and the liberal use of tonics. The influence of both tends to regulate the distribution of the blood—removes partial congestion and invigorates the action of the heart.

The arteries in every part of the body enlarge with exercise, but especially in the limbs. The free and repeated play of the muscles is accompanied with a proportionate expenditure of nervous energy and blood. The supply keeps pace with the demand. The nerves make additional

claims upon the brain and spinal cord, and the arteries to a corresponding extent upon the sanguiferous system. Both grow with the development of surrounding structures.

An interesting case is brought forward by Parry, in illustration of the action of the heart on venous circulation. The following are his remarks:—"A still greater influence of the heart is shewn under certain circumstances of disease; for I have already stated my having, on various occasions of inflammation about the wrist, seen, from a puncture of the lancet in the cephalic vein of the same arm the blood spring out in jets, as from a wound in the artery. And since, in one of those patients, in whom the same phenomenon occurred thrice, the cephalic vein was situated at least two inches from the brachial artery, and jets were synchronous with the pulse in the radial of the opposite arm, there can be no reasonable doubt, that each jet was the immediate effect of the accelerated wave of blood, propelled at each systole of the ventricle, through the capillaries and veins to the bend of the arm; that ventricle thus sensibly acting throughout nearly the whole circle on to the right auricle, without the smallest perceptible co-agency of the arterial, capillary, or venous systems."*

The condition of the blood in the vein, in this instance, is virtually the same as in the experiment of Magendie, which has already been brought under consideration. In this the blood was not allowed to flow into the body, but escaped from a puncture of the vessel, as in the ordinary operation of venesection. In both cases, the intermediate vessels are in a state of distension, precisely as a series of elastic tubes would be, previous to the ejection of water from the stroke of the piston. The bandage which

* Elements of Pathology and Therapeutics. By C. H. Parry, M.D., F.R.S. Second Edition. Appendix, p. 44.

is applied to the arm, preparatory to bleeding, is for the purpose of causing this distension, and in fact creates a resisting column, extending to the artery, so that the impulse of the left ventricle may possibly be transmitted to it. When the bandage is removed, the blood no longer escapes with force. It is not then propelled by the direct action of the heart, but chiefly by the capillaries. In the consideration of this subject, the writer had overlooked the important changes induced in the venous system by the application of the bandage, and possibly by the local disease.

The phenomenon certainly cannot be brought forward in illustration of the relations ordinarily existing between the heart and the contents of the veins. The relations induced exhibit the widest possible departure from what is normal. Whether the flow of blood, synchronous with the impulse of the heart, is satisfactorily accounted for on the distention extending from the artery to the punctured vein, it is for others to determine. It is clear that such will be produced, and the supposition that the heart, at this time, acts on the column of blood, as the stroke of the piston would eject fluid from any number of metallic or elastic tubes, will be regarded, at least, as probable. If the doctrine, which the writer endeavours to enforce by this particular case, viz., the influence of the heart on the current of venous blood, is imagined to be based on the natural conditions of the circulating system, it is singular that the evidence of it should not always be observed in venesection.

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PLAGIARISMS

OF

JULIUS JEFFREYS, F.R.S.,

PRICE SIXPENCE.

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Mr. Jeffreys, in a recent treatise, entitled, "VIEWS ON THE STATICS OF THE CHEST," has, without the slightest acknowledgment, taken the whole of the leading principles contained in two works of mine, one published in 1829 and the other in 1834. In answer to the charge, he says, that so far from being acquainted with my writings, he had scarcely ever heard of my existence, and as unequivocal evidence of the fact, he mis-spells my name in his first address to the public—an address disfigured by cant—bad taste and irrelevant matter.

He endeavours to show that my indignation is to be traced altogether to his connection with the Respirator. This is certainly a singular procedure on his part. My remarks on this instrument occupy about a dozen lines, in a pamphlet of thirty-six pages, exposing in detail his impudent plagiarisms. The manner in which he evades the charges, and the copious evidence on which they rest, lays open his motives to the most unfavourable construction.

The degree of confidence which his character is calculated to inspire, will be best determined by his actions. At the end of his work is a note, stating that many years ago, he printed an "Inquiry into the Comparative Forces of the Extensor and Flexor Muscles, connected with the Joints of the Human Body," but which was circulated only among friends. In this note he has the indelicacy to give the opinions of Travers, Abernethy, and Sir Astley Cooper, concerning himself, conveyed not to him, for he admits he was personally unknown to them, but in correspondence with others—his anxious and officious friends. Could these high authorities possibly imagine that their flattering and confidential remarks, wrung from them under peculiar circumstances, would afterwards be published by an individual to whom they were not

addressed? Is this conduct in harmony with a high tone of mind? Would any man, possessing a truly honourable feeling—one spark of dignified sensibility—extract from the letters of his friends, the kind and encouraging compliments of others—intended not for the public eye—and give them to the world as a sober and well-matured opinion of his merits? The act is indeed grossly indelicate, and shows a morbid appetite for distinction.

The treatise on the “Statics of the Chest,” contains many of the leading views and important discoveries of Liebig, to which he coolly lays claim, remarking that they “*had long had a place in his notes.*” Here is an individual, whose “reading of late years” has been devoted to “general and political science,” having in his notes, and for years, the most splendid discoveries of the age—the most fruitful in their practical results. What a wonderful note-book this must be, but how much more wonderful the owner, never to breathe a word of these discoveries, until they were announced by Liebig. Ho complacently says, that “doubtless each of them had been led independently to the same conclusions.” Doubtless they had. How vastly, however, the genius of Mr. Jeffreys transcends that of Liebig! The latter has passed a life in the laboratory, and his achievements have been the slow work of chemical analysis. To the former no such toil was necessary. The discoveries, in all their refined distinctions and beautiful applications, came into his head while studying “general and political science.” To him the researches of the laboratory would have been a waste of time—a useless expenditure of money. The discoveries cost him nothing—save the trouble of recording them in his note-book. The assumptions of this man have no limits—his assurance has no bounds.

The means which have been employed in making known the Respirator, have been one continuous and elaborate system of puffing. The tricks of Solomon, Lignum, and Morrison, have been played with unblushing effrontery. Ingenuity has been exercised in every possible way to procure testimonials, and these have been published in small books, and distributed through the length and breadth of the land. In many cases the testimonials have only the initials of the parties, some evidence of their unwillingness to be dragged before the public. Is such conduct as this usual with men to whose names is appended F.R.S.?

Is it in keeping with those high mental powers, which, without labour or chemical analysis, and while engaged in studying "general and political science," hit upon those brilliant discoveries, which Liebig arrived at only by the tedious process of thought and manipulation? Does genius, which merits immortality, descend to play the tricks of the quack? Does it at one moment soar in the highest regions of science, and at the next study how best to secure the pence of suffering humanity?

Do men who have made such discoveries, calmly say, when anticipated in the publication of them, "Doubtless each of us has been led independently to the same conclusions?" If such independence had been exercised in the investigations, is there not always some difference in the methods which were pursued—methods bearing the stamp of originality, and valuable to subsequent inquirers? Did he hit not only upon the discoveries of Liebig, but the very same steps of research? How curious the coincidence, but how much more curious the philosophical sobriety of the man, who, when thus anticipated, simply remarks: "Doubtless each of us has been led independently to the same conclusions." How little such conduct accords with that which induced him to extract from the confidential letters of friends, an inflated opinion of himself! If the suspicions which such an action must excite in every well regulated mind, disturb in no degree his self-complacency, he is indeed a greater phenomenon than his note book.

He pleads almost total ignorance of my existence. This may possibly be true, but it is somewhat strange. Has he not travelled to this town in the sale of his instruments, where my services, in connection with public institutions have made me well known? My works have been extensively reviewed in the leading medical journals—have been alluded to by eminent writers both at home and abroad. Are sought for both in North and South America, and have secured me the friendship and correspondence of men, not only in my own country, but in distant regions, to whom I have not the honour to be personally known. Is it not then singular, that an individual of the transcendant powers of Mr. Jeffreys—a keen and inquisitive inquirer—alive to the stirring events of science—entering warmly into the sufferings of humanity, and catching knowledge instinctively from every object around him,—that he

should never have stumbled on any notice of my works in the course of his extensive and multifarious reading? He states how anxiously he has consulted the first physiological treatises of the day, to ascertain the views of writers on the subject of animal heat, and has never fallen upon my name or labours. Is it not passing strange that he should refer to the *last* edition of Bostock's Physiology, in which excellent work, under the head "of Animal Heat," some of my views are mentioned, and yet should be ignorant of my existence. In Milligan's translation of Magendie's Compendium of Physiology, in treating of the temperature of the fœtus, my "Inquiries into the Laws of Organic and Animal Life," are alluded to in a complimentary manner. In the classic work of Edwards, "on the Influence of Physical Agents on Life," translated by Dr. Hodgkin and Dr. Fisher, my views on the important subject of animal heat, are discussed at length, and in the most liberal spirit. To the labours of Edwards he repeatedly refers. Did this translation and its valuable additions never fall under his observation? In the second volume of Dr. Southwood Smith's investigations into "the Philosophy of Health," the author treats of what he supposed to be my doctrines of animal heat. Did he never peruse this work, nor my corrections of the erroneous impressions of the writer, in the *Lancet*, No. 17, Vol. i, 1837-8? Have the standard works, equally with the periodicals of the day, been unopened during his devotion "to general and political science"? During that happy devotion in which he hit at once upon the splendid discoveries of Liebig?

He admits, that one person named to him in conversation, but *briefly*, the sameness between his views and mine. Was it necessary to be very lengthy in stating that the principles of one man are precisely like those of another? He never attempts to show that the views are not the same. He regards it as one of those peculiar coincidences which sometimes occur in the researches of great minds, as between himself and Liebig. But how unfortunate and pitiable is his case, that in all his discoveries he should be anticipated by others. In my case by fifteen, and by Liebig several years.

